DOCUMENT RESUME

ED 229 475 UD 022 755

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TITLE	The DTA Field Research Experience. Documentation and Technical Assistance in Urban Schools.
INSTITUTION	Center for New Schools, Inc., Chicago, Ill.
SPONS AGENCY	National Inst. of Education (DHEW), Washington, D.C. School Capacity for Problem Solving Group.
PUB DATE	Nov 79
CONTRACT	400-75-0018
NOTE	125p.
PUB TYPE	Reports - Descriptive (141) -- Viewpoints (120)
EDRS PRICE	MF01/PC05 Plus Postage.
DESCRIPTORS	*Educational Improvement; *Educational Research; Elementary Secondary Education; Experimenter Characteristics; *Field Studies; *Program Development; *Program Implementation; Research Design; Researchers; *Research Problems
IDENTIFIERS	*Documentation and Technical Assistance Project

ABSTRACT

This report explores the field research experience of the Documentation and Technical Assistance Project, an activity of the Center for New Schools in Chicago, Illinois. Using ethnography as a major research tool, DTA studied nine school sites, documented information on how the schools solved problems to bring about school improvement; and provided technical assistance in developing improvement strategies for other schools, based on information from nine sites. Chapter 1 of the report discusses the evolution of the project, and focuses on developments in research organization; research design preparation; hiring, training, and directing of site researchers; delineation of the site researcher's job; and relationships between researchers and the research sites. Chapter 2 examines constraints on the DTA research efforts, which might explain why the project evolved as it did. Among the constraints examined are: (1) the project's nonacademic context; (2) conditions of the research contract; (3) problems that arose from the project's collaborative arrangements; (4) the project ideology; (5) geographical dispersion of the project; and (6) problems in implementing the data management system. Chapter 3 analyzes emergent problems in the DTA field experience, which might be encountered in doing other field research. Appendices, which describe the role and responsibilities of site researchers, are included. (MJL)

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THE DTA FIELD RESEARCH EXPERIENCE

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David M. Smith

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November, 1979

UD 022 755

This paper is part of the Documentation and Technical Assistance Project of the Center for New Schools, 59 East Van Buren Street, Chicago, Illinois. This Project is funded by the School Capacity for Problem Solving Programs of the National Institute of Education, contract #400-75-0018. The views expressed in this paper do not necessarily reflect views of that agency.

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FOREWORD

The Documentation and Technical Assistance in Urban Schools Project (DTA), a project of the Center for New Schools, was an unusual and high risk effort to learn more about how to make urban public schools work better for urban students. DTA's mandate had three interrelated parts:

DOCUMENTATION - to study, using primarily ethnographic techniques, the ways in which selected urban schools organize themselves to solve problems. Documentation focused upon nine sites throughout the country selected and funded by NIE to continue to engage in locally initiated improvement activities.

TECHNICAL ASSISTANCE - to develop in practice specific strategies which were designed to improve urban public schooling. Technical assistance strategies were based in part upon documentation of useful practices in the nine funded sites.

LINKAGE - to increase knowledge of the processes through which research can be useful to practice, and practice can influence research.

DTA was partially terminated in January 1977 by its sponsor, the National Institute of Education at the end of Phase 2. NIE had decided that the project was not adequately meeting the high expectations for its productivity. Everyone agreed that the Projects' experience in grappling with a number of complex issues was rich and important, even if difficult. The Center for New Schools joined with NIE in wanting to maximize the value of what we had learned. Thus, in spite of the usual inclination to move quickly away from problems, in spite of the risks and (for some of us) the personal pain, we opened the day-to-day life of the project to systematic scrutiny and analysis.

This paper is one of the products of that process. Dr. David Smith and Dr. Lynn Gregory, both of whom worked on the project's research team, were asked to consider the lessons of DTA for the conduct of field research on education.

The paper does not officially represent the view of the Center for New Schools, nor does it necessarily represent my view as Project Director on the problems and lessons in the research effort. That was not its intent. The value of this paper is its contribution to the discussion of DTA's strengths and limitations relative to the development of viable and effective methodologies.

Other DTA papers which present different perspectives on the DTA research experience and may increase the reader's understanding of the issues we faced include:

1. S. Wilson. Explorations of the Concept of Local Capacity for Problem Solving. CNS, 1977. In the Methodology Appendix of this paper, Dr. Wilson, Director of the DTA Research Team presents the DTA research design and discusses some of its limitations and strengths.

2. T. Wilson. The DTA Final Report: Reflections of the Director on DTA Assumptions About Improving Schools, CNS, 1979. In this examination of the underlying assumptions about school improvement from which we built the DTA project, I consider the assumptions we made about research.

3. L. Smith and D. Dwyer. Federal Policy in Action: A Case Study of an Urban Education Project. CNS, 1979. In this case study of DTA, Dr. Smith, a DTA advisor, and David Dwyer examine the organizational dynamics of DTA in the context of Federal Policy for improving schools. Research issues are extensively treated.

4. M. Miles. Linkage in a New Key: The DTA Experience. CNS, 1979. Dr. Miles, an advisor to DTA, examines the project's work in directly linking research efforts with practitioners. From this context he considers several of the project's research issues.

Thomas A. Wilson
Project Director

INTRODUCTION

In recent years educational researchers have become attracted to a number of techniques for doing context sensitive field research that were developed within social science disciplines. Although some of these qualitative approaches have become so popular that they have almost taken on a faddish quality, their adaptation to the needs of educational research has not been without problems. The reasons are many. Some have to do with the uneasiness researchers more familiar with quantitative techniques feel in changing to the newer modes. Some of the problems have their bases in the demands on educational research in general, especially those occasioned by contracts or grants made by government agencies to carry out large and complex research projects. Some of the problems simply reflect the difficulties inherent in applying well tested research techniques to new settings.

In this paper we examine the field research experiences of one large, federally funded research project that attempted to use ethnography as its primary research tool. Nine full time site researchers worked for nearly three years on nine different urban sites to collect information on school improvement strategies in each site. These research efforts were coordinated by professionally proven supervisory staff and were supported by a number of secreteries, coders and transcribers.

Although a great deal of useful information was gleaned through this massive effort it was fraught with difficulties from the beginning and never lived up to the expectations either NIE (the funding agency) or the plann s and

executors of the project held for it. Since, however, the authors of this report are convinced that such efforts hold great promise for understanding and improving American education, we have attempted to trace and analyze the major developments of the DTA field research experience.

This paper is only one of several that have been written looking at various aspects of the DTA project, and thus makes no attempt to describe the whole project or even to describe the project set for the field research experience. Neither is this paper an attempt to describe in detail all of the field research. That would be a truly formidable undertaking. We have chosen a few of the developmental threads that can be isolated and traced throughout the history of the project. These have been described and traced in Chapter I. The result of this approach may be that the reader of Chapter One may feel that he/she is forced to jump into the middle of an unfamiliar setting.

Chapter II, which discusses a number of the salient features that constrained and shaped the field research, should serve to fill in the picture. It is hoped that the net result of these two chapters--a walk through of the important developments and a sketch of the major constraints on the project--will be a clear sense of what the field research looked like from an historical or developing perspective while not burdening the reader with too many unimportant details.

The final chapter analyzes what we consider to be the main issues in doing field research that have emerged from the DTA experience. While the setting in which these issues are examined is specific to DTA they are, in most cases, encountered in any field research project.

CHAPTER I: THE UNFOLDING DTA EXPERIENCE

INTRODUCTION

DTA was originally conceived as an experiment born of the frustration felt by the staff of the Local Problem Solving Group at NIE with the impotence of research efforts to bring about improvement in urban schools. The basic concept was simple. Hire a contractor with a demonstrated capacity for doing sensitive in-depth field research and work with the contractor staff to explore means for gathering information on urban education improvement strategies and then to develop techniques for applying these findings to educational practice in other urban settings. The contractor was not to be burdened with meeting narrowly defined, specific a priori defined objectives or with slavishly implementing previously proven research methods. Moving beyond the initial general goals, and developing the detailed study methodology were part of the contractor's charge.

In the words of the Project Officer (PO):

> ...both the government and the contractor began the work knowing that great risks were involved, that the project was extremely ambitious, that its specific goals would have to be shaped en route, that its methods would have to be invented, that it required an unusual degree of coordination of a far-flung and highly complex staff operation. We all knew the risks and chose to accept them. (Letter to reviewers of DTA products, February 7, 1978, p. 1.)

In light of this built-in evolutionary imperative, any useful analysis of the project must demonstrate a sensitivity to its changing nature and an understanding of the existential decisions that were responsible for each major development. In this chapter we "walk through" the DTA experience,

focusing upon those decisions that have emerged as significant in shaping the field research. We do not simply describe events, but also participant perceptions of them and their ramifications. The issues treated in the following chapter are drawn from this discussion. While this chapter is fairly DTA specific, the issues which it identifies are more global.

In tracing the unfolding of the DTA research experience, five developments are examined, each consisting of a series of program decisions:

1. Organizational changes that affected the research effort.
2. The evolution of the research design.
3. The evolving SR experience (hiring, training, working styles).
4. The evolution of the site researcher's job.
5. Developments in SR-site relationships.

Evolution of the Research Design

The original RFP did not specify what an acceptable research design should look like. Rather, it directed the contractors to "...spend three months gathering information from the sites in order to produce a detailed plan for the continued documentation of the funded projects." (Scope of Work, p. 11.) This plan was to include: a general framework or set of categories for describing the problem-solving process that would be usable in all sites and would yield comparable information; a specific focus within the case studies of each site; and specific methods to be used to gather information called for by the categories of the plan. (Paraphrased from the Scope of Work, pp. 11-12).

In their response CNS-CEPM submitted a long, complex discussion of their research philosophy. In this discussion they indicated that:

> ...overall research strategy would be guided by the general system theory of the sort explicated by James G. Miller and by the concept of grounded theory as explicated by Glaser and Strauss." (Response to NIE RFP, p. 3).

Significantly, in light of future staffing decisions, they went on to say:

> We reject the atomistic and mechanistic research strategies of R. A. Fisher, J. B. Watson and B. F. Skinner." (Ibid, p. 38.)

The proposal described three "conceptual frameworks" which would guide the research. These were entitled respectively: Key Concepts, Alternative Schemata for Connecting Key Concepts and Critical Activities. The first framework consisted of 46 concepts that would, presumably, serve as directives for collecting data, and as categories for organizing them. The second framework included the skills at the interpersonal, subsystem and school levels that are necessary for the establishment of problem solving capacity. "Critical activities" were a tentative notion about how to analyze experience to find essential aspects.

The discussion included a set of general questions that would be posed with respect to the funded sites and a description of the proposed staffing for the research team. The staffing was to consist of three research coordinators, one each from CNS, CEPM and the New York region; site researchers for each site; an advisor for each site, and, an advisory panel (composed of site practitioners) at each site.

It is clear from a perusal of the initial proposal that the contractors had taken seriously NIE's contention that research methodology and specific content were to develop through the doing. The document does not contain a carefully constructed research design. While a substantial set of guiding concepts is presented, the strategy was to hire qualified professionals as site researchers, research coordinators and advisors, to build in a great deal of close interaction among them all, to constitute an internal panel of people intimately involved and deeply knowledgeable about the local scene, and to let the research design emerge from this interaction. It was assumed

that the local interaction would insure the identification and investigation of issues important to each site and that the efforts of research coordinators and the co-directors would insure comparability in data. It was also assumed that the directors of the project, who were to give general intellectual direction, would be in constant contact with the whole team, keeping the apparatus oiled and functioning.

This plan was consonant with the kind of grounded theory approach espoused by the project. It was expected that the RC's and SR's, working in constant contact would zero in on the emerging analytic themes by approximately five to six months into the project. A series of cross-site foci would subsequently be formulated by the RC's. This plan was also the DTA equivalent of a research design. The DTA strategy was not to produce a written, imposed-from-the-top design specifying beforehand what data were to be collected and how they were to be analyzed. In its place they devised an elaborate process from which questions and their answers would emerge. "Research design" here, thus refers to the implicit framework underlying this process in the minds of the leaders of the research effort.

An unavoidable concomitant of this approach is that SR's had to continually face the existential decisions that are inescapable in designing field research. For DTA researchers these included: whether to focus on the total school context or the funded strategy, whether to be concerned with the comparability of data or with concentrating on unique features of sites, whether to work on case studies or to feed information to TA types, and whether to spend energy collecting more and more data or in formatting and analyzing findings for immediate use. The process was designed to resolve these tensions sui generis. However, to the very end, they remained sources of frustration and concern to the entire research team.

In the absence of advance answers to these questions, essential elements to their resolution along the way would be the hiring of qualified personnel and constant horizontal and vertical interaction focused on the work. As indicated in the following section, when it came to hiring SR's, practical and ideological considerations resulted in downplaying the importance of research skills. Given the structural and professional differences among the research coordinators, ideological commitments became an impediment to effective research issue oriented interaction among RC's and between RC's and SR's. For example, a conference called to train SR's was almost entirely devoted to dealing with questions of power and authority, with very little time given to actual training. The Woodstock conference, originally designed to formulate cross-site foci, was expanded to accomodate the prevailing notion that everyone should have an equal say in every decision, to the point that it was too unwieldy to concentrate its efforts on the original agenda items.

As a result, the original deadlines passed with no clearly formulated analytic themes being agreed upon to guide either RC's or SR's. No overall research design was ever devised or implemented.

The geographical dispersion of the project also had unexpected effects on the evolution of the research design. Due to the site's express desire for a Black coordinator, it was decided to appoint a separate RC for L.A. research, making the final shape of the research team as follows:

ORIGINAL ORGANIZATIONAL & RC RESPONSIBILITIES

Name/Location	Organizational Responsibility	Site Responsibility
Wilson/Chicago	CNS co-director of research	Minneapolis, Minn.
King/Chicago	Administrative assistant, CNS	Los Angeles, Ca.
Runkel/Eugene	CEPM co-director of research	Washington, D.C. Louisville, Ky. San Jose, Ca.
Babb/New York	Director of N.Y. operation	4 sites, New York, N.Y.

COMPARISON OF RESEARCH COORDINATORS

Ethnicity/ Sex	Sites	Training/Experience	Frequently Mentioned Research Agenda Communicated to SRs	Style of Relating to SRs and Sites
W/M	1	Ph.D (1972) Ed. Psychology Field research in alternative settings Some teaching in Ed. School. No prior knowledge of site	Uncovering problem solving capacity in schools not just TAG strategies. Developing analytical themes.	Colleague/friend model. Frequent and regular interaction--initiated by both parties. Voluminous suggestions--not pushed.
W/M	3	Ph.D. (1956) Social Psychology Extensive research/publication in O.D. University Faculty No prior knowledge of sites	Get each site's story Let Srs determine foci.	Laissez-Faire. Professor/Sr. Graduate Student Model Contact only on schedule or as unexpected developments dictated. Available for help but onus for asking on SRs.
B/F	4	ABD (Ed.D. rec'd 1978) Ed. Administration Public Classroom teaching Extensive knowledge of N.Y. City schools.	Explicate known problems of New York City schools, especially race and power politics.	Team captain model. Intensive, frequent interaction. Numerous suggestions--get everything possible analysis to follow. Protect SRs from Chicago/Washington pressures.
B/F	1	No graduate training. Community organization. No prior knowledge of site.	How to insure that community activist orientation of the TAG impacts schools.	"Movement" co-worker model. ("Sister"/"Brother") Provided many, suggested leads to be followed up. Frequent interaction--visits, letters phone. Liked being close to research--great deal of time on site.

Two important facts stand out from the above tables. First, none of the RC's could devote all of their time to that task. Secondly, each of them, with the exception of the New York RC, was located more or less distant from the site they were responsible for, and except for King and Wilson, were widely separated from each other. As a consequence, the kinds of intensive interaction planned among RC's and between RC's and their SR's was impossible to maintain.

A partial exception to this was New York, where the RC and the SR's did have frequent and intimate interaction. However, even here the research design did not develop as planned, partly because much of the RC's time was spent in administrative and other duties, partly because she was not involved in the original conceptualization and consequently brought her own notions of research with her, and partly because her own ideological bent toward treating researchers as professionals precluded imposing a rigid design on her SR's. Despite this lack of a shared, coherent research design, the New York region researchers came closest to forming a research team who were clear as to what they were about and who shared their findings with each other.

Changes in the starting dates for the research modified plans for developing a research design. It was originally assumed that the site researchers would be in place by March of 1975 and would have enough information to develop detailed research plans after three months. Complexities in negotiating site agreements and in locating, interviewing and hiring the researchers took much longer than anticipated.

As the following chart indicates, not only were the initial hiring dates all much later than planned, but in two cases the original site researchers left the job almost immediately, and new ones had to be found. Each

replacement required orientation which delayed the research planning process.

DATES SR's WERE HIRED BY DTA

Site	SR #1	SE #2	SR #3
Washington	4/75	10/77	
Minneapolis	5/75	7/75	
CCEC	5/75		
EDC	5/75		
San Jose	5/75		
Louisville	5/75		
Los Angeles	6/75	10/75	6/76
Open Education	7/75		
IS 162	7/75	7/77 (collaborating writer)	

A final thread of development important to determining the shape of the research design plans was evolving the data management system (DMS). In the original proposal, the contractors agreed to use a computerized system, assuming that a previously developed program could be fairly simply adapted for their use. This turned out not to be the case. None could be found that met either the technical or ideological requirements of the projects. Countless hours were spent in attempting to develop a program and a process that would work.

This not only set the planned research design plans back, but introduced unforseen complications in them. Some of the SR's resisted the requirement that field notes be prepared in a format that would permit their storage in the computer. Some were opposed, again on ideological grounds, to this use of field data. At least one of the sites imposed a ban on the transmission of field notes to the project on the field data forms until it had approved their format and use. For all of the site researchers, accomodating the DMS meant spending additional time in writing up field notes from that originally planned, time taken away from data gathering or preliminary analysis.

Our discussion to this point has only described the early problems of getting the process for developing a research design off the ground. The net consequence was that SR's went into the field with substantially less training in doing documentation and less orientation to the conceptual underpinnings of DTA than had been anticipated and than now appears to have been essential. Furthermore, the quality and quantity of interaction between the SR's and the rest of the research team was not what had been planned; at the same time their jobs were more complex.

Consequently, the hope that a research plan would gradually and "naturally" emerge from the early activities of the SR's and their interaction with RC's

never materialized. Instead an ad hoc process developed. This had several facets. At the beginning of Year One the SR's were asked to draw up and submit a research plan of their own. To facilitate this, the team leader and the RC's formulated a list of questions that should be addressed. A similar tack was tried at the beginning of Year Two, when site researchers were to simultaneously look at unique local factors, as well as common cross-site foci. In the latter case the design was to have been authorized by the RC's. However, the response to both of these attempts was less than enthusiastic, and the increased number of demands on the time of supervisors made it impossible to follow up these plans carefully enough to insure that useful designs were ever formulated or implemented at the site level.

Still another factor that affected this later stage of research design development was the attempt to meet the requirements of the technical assistance teams in the project. A major impetus of the entire project was to find innovative ways to get from research to technical assistance. DTA originally held high hopes for assistance from the computer system here, as well as in the research effort. One step in the originally conceived research development process was to have the technical assistants indicate to the researchers some of the kinds of information they could use. Accordingly, during Year One they prepared a list of questions they would like to have answered. At that time the site researchers were not in a position in their research to supply satisfactory answers or the sites were not seen as able to yield such answers.

Since this direct approach was largely fruitless, and since the TA staff were not finding the data in the computer particularly useful, the two teams went pretty much their own way. For the researchers, less effort was given to getting immediately useful information or in getting data coded and stored for immediate use, and most energy was directed to preparing case studies.

Not until near the end of Year One did anything resembling a research design emerge that all of the SR's were expected to follow. This was in the form of an outline for the case studies that was prepared by the director of the New York operation. At that time, however, the data collection was so far behind that the outline was unrealistic and was pretty much ignored. Later, in the third year, 1976-77, the new RC's worked out some approaches with each of the SR's. However, time was too short and the SR's were too invested in their original random and uncoordinated efforts to do the total rewrites that would have been necessary to conform in detail to these outlines. Furthermore, most of the local school improvement projects were near completion, and some of the data that would have been needed for the rewriting was lost.

To summarize, DTA never developed a comprehensive research design, despite the attention given the matter in the original proposal. Instead it proposed a process that would lead to the collection of data, their analysis and application to practice. This approach was seen as in keeping with the ideology of the contractor, the requirements of the grounded theory method and the exploratory tenor of the RFP.

This process was predicated upon several key features. These included the hiring of researchers who could be self-directed (through training in qualitative research methodology or other impetus), close vertical and horizontal interaction on research issues among members of the research team, intimate involvement in the whole process by a director of research who had a strong personal sense of direction, philosophy and goals, and lastly, the presence of a functioning data management system to support the intellectual work. None of these key expectations were wholly met.

The two chief results were: (1) that the project's emphasis shifted from the collection and analysis of comparable data to be used to help

others, to the preparation of case studies telling the story of each site; (2) as the supervisory structure failed, prime responsibility for data gathering, analysis and writeup was left to the SR's, with the consequence that the products were of widely varying quality.

Organizational Changes That Affect the Field Research

As expected in a project of DTA's magnitude and complexity, a number of changes in key personnel and in definition of key roles took place throughout the existence of the project. While each may have affected the research to some degree, three major organizational arrangements can be identified that reveal important aspects of the evolution of field research activities. Each resulted from or assumed a change in research strategy or emphasis.

The original structure was organized around project tasks and was designed to insure equality of input and responsibility between the two contracting centers. The two research tasks were: Task 1, documentation, and Task 2, analysis. The CNS co-director of research had primary responsibility for Task 1, and his counterpart at CEPM for Task 2. Both were members of the team responsible for Tasks 1, 2 and 3 (technical assistance to user sites), and were supposed to actively participate in the other four tasks as well. Each co-director also served as an RC, with the CNS representative overseeing one site and the CEPM representative three. A third RC, located in New York, oversaw all four SR's there, and a fourth, located in Chicago, had responsibility for the Los Angeles site. Each of the RC's had other obligations in the project as well.

By the fall of 1975 the work overload occasioned by the unanticipated problems of getting the project started and the frustrations with the role ambiguities attendant upon the requirements of keeping abreast of the several different tasks for which each staff was responsible, resulted in plans to change the organizational structure. Furthermore, by this time the strengths

and interests of key staff members had become clearer, as had the problems associated with coordinating the site researchers and other tasks from both Oregon and Chicago.

Accordingly, in the project's Response to Questions Regarding DTA's Progress and Plans submitted to NIE at the refunding point of November 1975, a new structure was proposed. Three teams were to be constituted, each with responsibility for one of the major thrusts of the project and each under the direction of a single individual. The documentation team, with staff from both units, was headed by Wilson, the original co-director of research from CNS; the technical assistance team (including staff from both Oregon and Chicago) was to be the responsibility of Schmuck at Oregon, already one of the overall project co-directors; the study team, charged with studying entire linkage and assistance process, was headed by Moore at CNS, who had been involved in Task 3.

Under the new arrangement, the four research coordinators would continue to function much as they had before, with King taking on major responsibility for implementing the data management system and relinquishing her role as administrative assistant to the CNS principal investigator. Their emphasis shifted from simply supervising the SR's to analyzing their data in the formulation of cross-site foci. Only in New York, where she was on site, did the RC, in fact, continue to supervise and coordinate the efforts of site researchers with anything approaching the sustained level of interaction and support originally planned.

While this move was seen as a step in streamlining the organization of the project and reducing the original role ambiguities in the interest of more efficient management and clearer responsibility for leadership in the three central tasks, it reflects a departure from an original idea of

not in any way separating research from technical assistance. The underlying democratic ideal of allowing full participation in all major decisions by all staff was still heeded as much as possible, and the collaborative ideal was maintained by having staff from both groups expected to work together within the three teams.

Despite a number of staff changes and de facto role redefinitions by incumbent staff, this structure was kept in place throughout the entire second year of the project, 1975-76. Significantly, this was the year of a second review by outside reviewers, NIE assessment of progress in the anticipation if future funding and of transition from Phase I to Phase II.

Partly as a result of the NIE/outside review of DTA progress, and partly due to new shifts in emphasis during Phase II, the project was reorganized again in the summer of 1976. Collaboration weakened, as two separate TA efforts were established, one in Chicago and another in Oregon, and it virtually ended as the Oregon RC dropped that role and transferred all coding and case-study responsibilities to Chicago staff. The New York group became even more autonomous than before, with establishment of their own analytic agenda distinct from the others.

In response to the reviewers' suggestion that a more centralized and concerted intellectual attack be made on the project's research issues, two new RC's, one with Ed.D and the other a Ph.D. in anthropology, and both with school research experience and demonstrated writing skills, were hired to be based in Chicago. A part-time assistant to the DOC team leader was also promoted to the position of RC. The new structure thus consisted of the leader of the DOC team and three RC's, with responsibility for four, three and two (part-time) sites respectively. All were based in Chicago, where the data management system and support staff were located.

The implications of this reorganization were far reaching and profound. It represented the final capitulation of the attempt to equally distribute the research, TA and linkage activities between the two centers. Although care was exercised to insure that the new staff were ideologically compatible with the DTA (CNS) mission, the desire to bring in staff who could analyze experience resulted in the hiring of highly trained university types which, unexpectedly, signalled a shift in the research component. In the absence of any clearly articulated guidelines for cooperatively analyzing and synthesizing the field data, the new staff fell back on the academic model of writing papers on their own interests. Indeed, the new RC's' time was to be almost equally divided between supervision of SR's as they prepared the case studies, and writing analytical papers of their own.

These changes had important implications at the SR level, as well as on the final research products. By the time the new staff was in place, the original date for the draft versions of the case studies had already passed (11/30/76), and, if things had gone according to plan, the SR's would have been rewriting them for final submission in September 1977. The process, however, was seriously behind. Nevertheless, the project felt great pressure to get the case studies done so that they could be reviewed to give NIE some basis for continued funding. In addition, the papers assigned the new RC's were causing pressure for the same reason--NIE was insisting on results by early fall 1977 as a basis for continued funding.

As a result, the new RC's, unhampered by all of the ideological baggage of the early project, helped the DOC team take a hard look at the qualifications and progress of the SR's. Several important decisions were made. At one

site the RC decided that, not only did the SR not have the writing skills necessary to get an acceptable case study done in time, but that, after the fact, he had not collected or analyzed enough data. He was replaced by a researcher who had to depend on interviews and document perusal to write the case study. In another case, the RC decided that the theoretical stance of the SR had led to his insensitivity to the richness of the site, and the RC seriously revised the case study, adding a dissenting chapter of his own. (Ultimately, this process led to a dispute with the SR, who withdrew his authorship.) In a third case, a writer was hired to work with the SR in the analysis and final write-up of her data, with the SR ultimately withdrawing completely. In a final case, three successive SR's contributed an aggregate of no useful data, so the RC found it necessary to conduct his own investigation--post-project interviews, site visits and document search--and to write an abbreviated case study himself.

Despite these dramatic steps taken by the new staff of the documentation team--staff much more oriented to "traditional" research--none of the case studies were finished in time for review by NIE at the end of Phase II. Furthermore, each of these moves represented a tacit admission that the original approach to field research in DTA had serious shortcomings.

The following chart recaps the major changes in the organization of the DTA field research effort.

DTA ORGANIZATIONAL CHANGES AFFECTING FIELD RESEARCH

	Key Personnel	Roles	Location
First Year Task Oriented 1/75-11/75	S. Wilson	Co-director of Research (CNS) Head Task 1 Task 2 & 3 team member RC (1 site, Minn.)	Chicago
	P. Runkel	Co-director of Research (CEPM) Head Task 2 Task 1 & 3 team member RC (3 sites, D.C., Ky., Ca.)	Eugene
	Y. King	Administrative Asst. to PI RC (1 site, CA.)	Chicago
	T. Babb	Director, N.Y. Operation RC (4 sites, all N.Y.)	New York
Second Year DOC Team: Dispersed 12/75-11/76	S. Wilson	Leader DOC team RC (1 site, Minn.)	Chicago
	P. Runkel	RC (3 sites, D.C., Ky., Ca.)	Eugene
	Y. King	Development data management system RC (1 site, CA.)	Chicago
	T. Babb*	Head N.Y. Operation RC (4 sites, all N.Y.)	New York
Third Year* DOC Team: Centralized 12/76-12/77	S. Wilson	Leader DOC team	Chicago
	A. Hyde	RC (3 sites, N.Y., Ky., D.C.) Assistant to DOC team leader	Chicago
	N. Johnson (-6/77) (D. Smith-- 6/77-12/77)	RC (4 sites, 2 N.Y., 2 Ca.)	Chicago
	D. Baldwin	RC (part-time, 2 sites, N.Y., Minn.)	Chicago

* T. Babb remained Director of New York Operation, but not RC

The Unfolding Site Researcher Experience: Hiring, Training and Directing of Site Researchers

Another important developmental thread, characteristic of, but not unique to DTA, involves the hiring, training, and directing of site researchers. The way DTA staff handled these tasks shows the strong infulence of a set of attitudes and a preferred style of working.

Hiring. The project ideology and commitment to grounded theory had serious impact on the hiring of SR's. Insuring fidelity to a grounded approach meant that the "uniqueness" of each site would have to be captured, and propositions about cause and effect derived from a base of observation at each place. In recruitment this was translated into concern to find researchers who had educational and professional backgrounds perceived as compatible with particular sites. To insure this, both TAG's and the schools at each site, as well as the NIE project officers, were included in the recruitment and hiring process. Although DTA retained the right to make the final decisions, these other parties had considerable input, and in some cases exercised a virtual veto.

DTA drew up a list of general qualifications that they would look for in addition to basic compatibility with the site to be studied. Site researchers would be expected to show the following characteristics:

- They should themselves want to learn more about schools.
- They should take a helpful stance, not an evaluative stance, toward the target schools.
- They should be able to understand easily the goals of the project.
- They should be skilled in a variety of data-gathering methods, and understand the basics of validity and reliability of data.

- They should be able to write clearly.
- They should be able to build trusting relationships with their own staffs, with target schools, with helping agencies, and with local evaluators.
- Each should be a member of some ethnic group that is significantly represented at the site. This may not be important at San Jose.
- They should be interested in moving into the work with the user schools, and should be likely to be available at that time.
- They should not have special histories that make them unwelcome at the site.
- They should be educated in a relevant social science.
- They should have had some experience as teachers.

(DTA proposal, pp. 80-81.)

Although they didn't realistically expect to find such paragons, the job availability announcements that were issued did insist on: (1) skills and experience in qualitative and quantitative social science research, (2) experience in urban schools, (3) sensitivity to issues of rapport building, and, (4) graduate training in education, anthropology, sociology or psychology.

Another quality looked for in the total set of site researchers, but not explicitly listed in the job availability memos, was a range of ethnic and racial characteristics. In a circulated "notice of positions open" applicants were informed:

> It is especially important to us to obtain minority persons with backgrounds similar to the minorities at the nine sites. Applications from women and the handicapped are encouraged.

The decision to ask for candidate suggestions, and ultimately to seek the nomination of acceptable persons from site staff, [illegible]rved to make the

the, SR hiring process much more complex than originally anticipated. While the rationale for DTA's list of qualifications was fairly straightforward--they wanted to make sure of getting accurate information which would be used as the backbone for the entire project--site people expressed concern. The DTA term, "documentation," was frequently assumed to be some form of evaluation, despite repeated statements by NIE that that was not the contractor's charge. In any case, local project people wanted to be sure that they would be accurately described.

DTA ideology supported another concern, that the information gained from the documentation not simply be "stolen" for others' benefit, but be fedback into the project so participants "could learn from it." With this end in mind, site people sometimes looked for researchers who had some understanding of (and sympathy toward) the ideas and activities of the project.

In a project dealing with early childhood education, for example, the director actively lobbied for someone who knew this area and who would be philosophically sympathetic to its ends. In one project using organizational development techniques the project directors sought someone familiar with these particular techniques. In another case, where the project was directed by a man who characterized himself as a "Rogerian humanist," the director supported hiring a "Skinnerian behaviorist" because he felt that it would be a good test for his program to be looked at critically from another perspective.

The DTA attempt to attend to the many concerns of school people and others in the sites, and occasionally NIE concerns, while at the same time

insuring that its own interests were met, resulted in protracted discussions, complicated by distance. The nine site researchers ultimately hired had widely varying experience, training, perspectives and ethnicity. (See Table I following.) Of the seven categories of qualifications important to the project, none of those hired satisfied all. Staff ability in the seven areas can be summarized as follows:

1. Skills and experience in qualitive or quantitative research. Only three of the nine original site researchers had experience in qualitative field research. These three all had M.A. degrees in anthropology and had done some ethnographic research. One of the others had conducted a modest field research project in conjunction with a graduate research methods course. A second had experience in observing schools as an investigative journalist. A third was experienced in quantitative laboratory research. The remaining three had no research experience at all.

2. Experience in urban schools. Reflecting the DTA primacy of experience, all but one of the site researchers had had some experience in urban schools. However, it varied greatly in quality. Two had been teachers, two had served as substitute teachers, one had been a student teacher, and two had worked in administration.

3. Familiarity with project locale. In all but two cases, where the SR had had experience in an urban school it was in the same city as the funded project. The two who had had teaching experience had worked elsewhere. All of the SR's were either natives of, or had been located for some time in, the general project area at the date of hiring.

4. Graduate training in a related discipline. Three of the researchers had graduate training in anthropology, two in education, and one each in social work, sociology, and psychology. The ninth had no graduate training.

TABLE I CHARACTERISTICS OF THE HIRED SITE RESEARCHERS

FUNDED PROJECT	RESEARCH EXPERIENCE	EXPERIENCE IN URBAN SCHOOLS	FAMILIARITY WITH PROJECT AREA	GRADUATE TRAINING	KNOWLEDGE OF TAG: CONTENT OR METHOD	PERCEIVED ABILITY TO BUILD RAPPORT	ETHNIC/RACIAL AFFILIATION
CULTURAL CONTINUITY PROJECT. Self help project in a Black Community (Los Angeles)	Non-qualitative sociological (Originally Hired SR.)	Some.	Resident of the project city.	Sociology	Good-staff member of the contractor.	Yes	Black
FACULTY DEVELOPMENT PROJECT. Teachers union initiated. (San Jose)	Anthropological and sociological in South America and urban U.S.	Taught rural and suburban schools.	Minimal. Resident and student in the San Francisco Bay area.	Ph.D candidate in anthropology and education.	Good. Did Ph.D research on similar issues.	Yes	White
IMAGINATIVE EDUCATIONAL COOPERATION PROJECT. Organizational development based on Rogerian humanistic Psy. (Louisville)	Yes. Skinnerian behavioral psychology laboratory research.	Worked as an assistant advisor in Follow-Through program in Louisville.	Yes. Resident of the project city	Ph.D candidate in experimental psychology.	Indirect. Worked with parents to identify kids.	Yes, despite different theoretical orientation. (Thought it would bring a different and useful perspective.)	White (Project orginally specified minority.)
INNER CITY CONSORTIUM--SOUTH-END SCHOOL. A primarily Hispanic student population. (N.Y.)	No (Originally hired SR.)	Hispanic community organizer who was frequently involved in educational issues.	Although native of N.Y. had no previous familiarity with neighborhood of project school.	None.	Minimal, at best.	Yes	Hispanic (Puerto Rican)
JUNIOR HIGH NETWORK PROJECT School Improvement using MBO. (Washington [illegible])	No (Originally hired SR.)	Substitute teacher, urban social work.	Native of project city.	Master's in Social Work	Minimal	Yes	Black
NEIGHBORHOOD CORPORATION FOR EDUCATIONAL IMPROVEMENT. Hispanic parent and student population (N.Y.)	Yes. Antropological. Both rural and urban field work in Mexico.	Taught high school in Albany, N.Y. Taught all grades in Mexican Catholic schools.	[illegible] citizen. Knew Hispanic Community. Resident of N.Y.C.	[illegible] candidate in anthropology.	[illegible]	Yes	Black
SATELLITE TEACHER CENTER PROGRAM. (Minneapolis)	Yes. Antropological field work in an urban center.	Some student teaching. Administrative intern.	Minimal. Resident of project city.	M.A. Anthropology	Minimal.	Yes	White
RESPONSIVE EDUCATION PROGRAM. Early childhood education. (N.Y.)	Yes, as a journalist.	Intern in open classroom techniques in city schools.	Native of India, but resident of New York City.	M.A. Journalism M.A. Early childhood education.	Some familiarity with open corridor program and knowledge of child development.	Yes. By virtue of journalistic training seen as "open".	Indian
SCHOOL SELF-IMPROVEMENT PROJECT Use of O.D. techniques in school administration. (N.Y.)	Only as incidental to graduate courses.	Educated in the N.Y.C. schools, served as substitute teacher in N.Y. Jr. high school.	Minimal knowledge of school neighborhood. Native of N.Y.C.	Ed.D. Candidate in educational administration.	Familiar with organizational Development from graduate school.	Yes, but the TAG voiced concern that she was hired because of race alone.	Black

5. Familiarity with content or methodology of project. In three cases this was an important criterion for hiring, and in a fourth, the researcher had done his Ph.D. dissertation research on a problem indirectly related to that being addressed in the project.

6. Perceived compatibility (potential for establishing rapport). In some sense, this qualification is different in kind from the others. It was the sine qua non for hiring and represents the best judgment of DTA, based on an evaluation of the other qualities in the context of each individual site's requirements. By definition every SR hired was seen as capable of building rapport. However, in several cases the project knew it was taking some risks, either with respect to an outside group or the school. In one instance, for example, the school was well-satisfied with the SR, while an outside helping group had reservations from the beginning. In another case where everyone recognized that the project and the researcher represented opposing theoretical positions, people explicitly agreed that the level of expertise demonstrated by the researcher and his professionalism would overcome any problem.

7. Ethnic/racial compatibility. In three instances where individuals of a particular ethnicity were requested by the site, DTA was able to comply. In two others where minority researchers were requested non-minority individuals were hired without incident. In the remaining cases this was not an issue.

From this summary several observations suggest themselves. Overall, of all the qualities orginally looked for, SR's were weakest in the area of qualitative (or other) research experience. This is consonant with the project ideology and its plans for on-the-job training. The most important

hiring consideration appears to be prior experience in urban schools, and in particular, knowledge of some aspect of the site to be documented. There are several explanations for this. Both DTA and the sites were interested in practical uses for the field work, in the anticipated experiments with technical assistance by DTA or feedback to the site being studied. These hopes for the future, coupled with a wariness about the usefulness of past social science-based research, led to the conclusion that practitioner-oriented, or at least practitioner-sympathetic, field workers would be of more use. In addition, DTA believed that selection of staff acceptable to sites was important to making solid entry and building good rapport. This sensitivity was not without good cause, as several of the sites from the beginning expressed reservations about the whole documentation effort, and viewed the money spent on this effort as being denied them.

Training. Given the relatively weak research skills of most of the SR's, the planned training was crucial. This training was to have taken several shapes, the most important of which was the built-in apprenticeship experience the researchers were to have. While the SR's would be on the line to produce data from the beginning, they would not be alone. They would be in weekly contact with RC's, who had their own experience to draw from, and who would be constantly informed by conversations with their counterparts supervising research in the other sites. They would have a local advisor they could look to, and there would be occasions for them to interact with other SR's. Additionally, to inculcate more technical skills, such as coding for the computer, and to orient SR's to the larger project picture, special training sessions were planned. At these conferences outside experts in research methodology would be used. In addition, senior project staff and outside

consultants would make frequent trips to the sites to work first-hand with the researchers on any problems they had.

Few of these plans actually materialized. Problems with the data management system, the limitations of long distance telephone interaction, and the failure to locate committed site advisors with time to devote to the research all contributed. The formal training process had to be drastically modified as well. The first SR conference was held in July 1975 in Oregon. The co-directors of research and the RC's had met prior to this to plan the agenda, which was primarily given over to training in data coding. Harry Wolcott, a well-known and respected anthropologist with experience in schools, was invited to participate in the week-long conference.

During the first day of the conference the SR's, several of whom had just been hired, requested that they be permitted to caucus by themselves. They came back insisting that the original agenda be abandoned and as a consequence the conference was given over to efforts to build a group consciousness and carve out a territory in the project for SR's. The proposed training was forgotten. This became a general pattern. Subsequent training conferences were similarly derailed as SR's pressed supervisors with specific problems with salary or perks, coding, travel, etc., and with questions of insuring their participation in the project as a whole.

No structured training program ever materialized. This was partly rationalized after the fact by project ideology. The notion that all SR's should be treated as professionals was seen as being in conflict with requiring them to undergo extensive training. The decision not to force the issue was partially rationalized by a fear of impeding the process of building grounded theory, coupled with a fear of "laying any more on" the already

overburdened researchers.

The consequences were not all negative. Most of the site researchers did gain an understanding of and feel for their own individual sites. Virtually all of them had interesting and potentially useful stories to tell.

The weaknesses of this approach were that few comparable data were gleaned and that they were of variable quality since documentors had no general standard by which to judge them. Furthermore, glaring weaknesses emerged when it came time to write them up. Few of the site researchers, including some with prior research experience, had ever had to face the production of a piece as comprehensive as the case study was expected to be, and it turned out to be a frustrating writing experience.

A number of measures were created to make up for the recognized shortcomings of the training process and of the problems with writing, on an ad hoc basis. As a general rule, when an RC, or the directors of research, flagged a problem they would visit the site, confer by telephone, or correspond by letter directly with the SR. RC and SR correspondence during the project was voluminous.

In one New York site, the RC hired an anthropologist to tutor the SR in ethnographic methods. For some time he worked closely with her, going over her material with her and accompanying her to the site. In other sites the project allowed funds for SR's to hire advisors from local universities to work with them on particular problems of data collection and analysis. The CNS library in Chicago collected a large number of journals, books, and papers on educational research and field research methodologies. These were made available to the SR's as requested or suggested by RC's.

When the time approached for writing the case studies, even more substantial means were implemented to aid the SR's in their work. On two sites where serious analysis and writing problems were quickly apparent, writers were hired to collaborate with the SR's in finishing the studies. In one of these cases, this arrangement was abandoned in favor of hiring an experienced researcher to take over, and in the other, the writer gradually replaced the original SR, who withdrew. In two other sites a writer was hired at the very final stages to help in organizing and writing up the final drafts. For two of the sites the RC's spent many hours working with the SR's to rewrite and reorganize their material, and, as has already been mentioned, for one site the RC collected his own data and did the actual writing after three successive SR's produced little of use.

Nevertheless, while the experience was in many instances frustrating and tedious, virtually all the SR's, when interviewed after the end of the project, volunteered that it had been a rich learning experience for them. At least two have used, or are using, their research and findings as bases for Ph.D. dissertations.

Working style. Closely related to the hiring and training of SR's is the working style that evolved in the project's documentation team. This is reflected in several aspects of the research--SR field style, relationship between RC's and SR's, DOC team superstructure style--and from several perspectives.

A fairly clear picture of the SR's and their working context has emerged from this discussion. Most of them were relatively untrained in research methodology. They had been thrust into the field with relatively little training. For most of them the fields were familiar, but the tasks were not.

They were asked to "document." What this meant was unclear for several reasons. Documentation was neither evaluation nor was it typical social science research. Their role was frequently referred to as "ethnographer," but few really knew what this was and those who did felt that the description did not fit. They were to document sites and yet many "sites" consisted of at least two formal organizations--a group of educators and parents in the school or schools, and another group in central office or elsewhere helping the first group. They were to take field notes and then "code" these into categories they had not designed and, in many instance, did not understand. The project management professed, and indeed tried, to treat them as "professionals," instead of employees, and this was extremely uncomfortable. They were not able to contribute as professionals nor were they free to ask for the kinds of help or admit to the ignorance considered appropriate to employees. They had been introduced to the RC who would be supervising them, and others they could call on for help, but they didn't really know them, their styles or expectations.

SR-RC interaction was to have been frequent, but the press of other duties for RC's and their attempt to respect the "professionalism" of SR's left much of the burden for communicating on the SR's. This was not only burdensome and frustrating--RC's weren't always easy to get on the phone--but some of the SR's literally didn't know what to ask much of the time. One of them expressed this problem candidly in an interview:

> My RC would only call about once a month. I could call him, but he was hard to get. This was only one of the things he had going... I never understood the coding, and I was always confused as to what we were to focus on. I saw that none of the other SR's knew either, so I didn't ask about it. I just assumed this kind of confusion was normal.

As a consequence of all these factors, most of the SR's, in their relative isolation and in face of the ambiguities of the research design, fell to doing what they felt comfortable doing. The experienced researchers looked at the issues and phenomena they thought were importnat. The inexperienced ones attempted to "go by the book," that is, follow the directions of the site research manuals and respond to the voluminous memos and requests coming from Chicago, Eugene and New York.

The Evolution of the Site Researcher's Job

A fourth strand that can be traced in the unfolding DTA research experience is the evolution of the SR's jobs. As the effort progressed, important changes were made in the SR's' roles in DTA and in assigned responsibilities.

Role in DTA. In the original CNS-CEPM response to the RFP, the SR job was described as follows:

> This person will oversee all work at the site. Duties will include making visits to the target schools and the helping agency, training and helping the panel of Inside Researcher-Advisors, helping to do premlinary processing of the data...and keeping close liaison with the Research Coordinator.
> (Response to RFP, p.77.)

This is a deceptively simple description. Proposal writers expected the SR's to be key components of the entire project. They would determine the shape of and control the flow of information on which the analyses and TA efforts would be based. They were to be sophisticated receptors and information conduits more than analysts or shapers of policy for the project, as might be predicted when "reasonable expectation of establishing rapport" came to be the most heavily weighted hiring qualification and research skills the least weighted.

Ironically, that was the role they should have played, had the rest of the plans worked. But partly due to their own insistence (cf. the SR caucus incident at the first training conference) and partly due to the other developments in the project that have already been described, their role was quickly expanded. The breakdown in the planned development of a data management system that would have permitted third party analyses of their data, resulted in increased dependence upon them for analysis. Attempts at "living" the ideology of democratic decision-making led to allowing and even eliciting their input to major decisions. Although the actual substantive impact their opinions had is moot, this greatly complicated the process, if in no other way than by introducing more "noise" in the system, because the SR's differed among themselves in concerns, politics, tolerance of ambiguity, etc. For example, the New York group held one set of views honed in extensive interactions, two other SR's saw themselves as anthropologists, another wanted to be seen as "Mr. Nice Guy," etc., etc.

Despite these changes in role definition, the preoccupation of RC's and other higher echelon project staff with problems and issues other than field research resulted in less SR-project-as-a-whole interaction than originally anticipated. Most of the SR's felt lowest on the totem pole and relatively ineffective in shaping the project. In interviews at the completion of their part in the project, five SR's characterized themselves as "merely employees" who simply did assigned tasks. The others attempted to view themselves as semi-independent researchers, having done what they wanted to within the limitations of the project.

During the project, framed by the initial role definitions and those that finally emerged, another shift in definition was tried. At one point it was decided that the SR's should serve as the direct links between the

field research and the technical assistance. Members of the TA team were encouraged to contact the SR's directly. During this time at least one of them gave a series of workshops to a user school on the strategy she was documenting. This direct linking role was later abandoned, since the SR's felt already overwhelmed with the demands they were facing (See Appendix B) and because, from the TA team's perspective, their findings were of limited practical use to the technical assistance strategies that had been developed.

Some of the more experienced, academically-connected SR's played the role of outside spokespersons for the project, organizing sessions and presenting papers at professional meetings. These were informal, non-official functions only incidently related to their roles as SR's.

Job assignments. Wherever they pegged themselves on the professional-employee axis, SR's all faced the problem of dealing with an ever growing workload, complicated as the project's procedures, structure and personnel changed, which resulted in the assignment of tasks that sometimes appeared frustratingly contradictory.

They had to deal with the demands of their role as researchers, such as keeping in touch with major events at their sites, both the TAG and the school(s), interviewing people, locating and searching documents, negotiating entry and explaining their presence, soothing ruffled feelings, allaying suspicions, touching base with all sides, tediously writing up field notes, keeping logs and just hanging around to establish rapport. They had to wrestle with the unfamiliar tasks imposed by the DTA data management system--learning and understanding the changing coding system, filling out FDF's or later "analytical logs." They had to cope with administrative requirements of the DTA bureaucracy, such as recuiting, hiring, and invoicing for site advisors, sending in bi-weekly time-sheets, and mastering the researcher manual.

They were encouraged to prepare themselves professionally by reading extensively from the list of books and articles supplied by the project, and occasional papers suggested by the NIE project officer and others. They had to host countless visits by DTA personnel visiting the sites and outsiders who wanted to see what was going on. They had to prepare for and attend a number of DTA conferences and then prepare reports on these events. They were expected to read every major project policy paper and react to them, even when these policies did not directly affect them or their work. They had to prepare for and submit to periodic evaluations of their work. They were asked to respond to requests for information from members of the technical assistance team and others, doing research or analysis in related areas and in a few cases, workshops. They had to host and make an extended cross-site visit, and then prepare a report on it. They were scheduled to write a short analytical paper every two months, which would be ready by the RC's and then discussed. Ultimately, they were to prepare a case study (one version by November 1976, after a year and a half in the field, and a revised, updated final version by September 1977). During all of this, they were to keep in constant touch with their RC's. (A copy of a memorandum from the co-director of research and the RC's listing the schedule of expectations as of September 1975 is appended.)

While this could be viewed positively as defining a rich, serious and demanding job, a workload of this magnitude was not envisioned in the original plans and required the setting of priorities and cutting corners by the SR's. These decisions may have been frustrating for the experienced SR's, but they were not insoluble. The inexperienced ones, who felt they had to "go by the book," simply could not handle the load, with the result that relatively

superficial data were collected and very little first level analyses carried out until the pressure of the case study.

For the project as a whole, the workload was demoralizing and resulted in everyone working with a sense of "being hopelessly behind." A more important consequence was that RC's, and ultimately technical assistants, were seldom dealing with current information, so that suggestions for refinement or attempts to fill in lacunae were often futile. In addition, the SR's who were more dependent upon RC's for direction were sometimes left with periods of relatively wasted time while they waited for new instructions.

During Year Two of the project, as the earlier data management plans were modified and as less real effort was being made to directly inform technical assistants, SR's focused more and more on the preparation of the case studies. It was tacitly recognized that these were going to be the major deliverables of the field research effort. In July of 1977 a research coordinator was hired (the present author) as a replacement for one of those taken on the preceding December, with sole responsibility for completing four of the case studies. This new emphasis on case studies represented a major shift from earlier project strategy. In the initial conceptualizations, analyses of experiments with novel techniques for informing practice from research were to have been the primary products. The case studies were included almost as an afterthought by the authors of the RFP to assure some tangible product. It was assumed that they would be the natural outgrowth of the research effort, perhaps an edited compilation of the other SR products.

Summary. Like other aspects of the project, the SR job underwent a significant evolution. This development can be traced on two dimensions: the SR role in the project and the increasing workload. Originally the SR's' primary role

was conceived as liaison between the project and the sites and as conduit of information. This conception clashed with facts of project ideology and proved unrealistic given structural and procedural changes. Ultimately, site researchers were expected to work alone on products much like those of traditional social science research, with occasional project decision-making functions. Some of the SR's were ill-qualified for these new role demands.

The original task assignments were not carefully specified, but as the role was redefined the workload grew until it was unrealistic. As a consequence, decisions were made to focus on the case study, a shift away from original project emphasis.

Developments in SR/Site Relationships

From the beginning, the DTA management was concerned that the site researchers be able to strike a delicate balance between objectivity and sensitivity to the activities and perspectives of the groups they were studying. They were aware that the goal of grounded theory development would place a heavy burden on the SRs is maintaining such a balance. The Site Researcher Manual (June 1975) makes this clear.

> This kind of research is fraught with the dangers of nonobjectivity and non-standardization. Because there are no standardized instruments much is left up to personal choice. Traditionally ethnographers have guarded against these dangers by applying rigorous standards of introspection to their work, and you will be expected to follow these canons of self-criticism. In addition, you have built-in processes of data analysis and interaction with other people in DTA which should help you guard against these dangers.

The role of the site researcher was labeled "non-participant-observer." The parameters of that role were laid out as follows:

> The site researcher will be neither a detached outsider nor an active participant in the...project. The site researcher will develop a sufficient understanding and sympathy with the...project and the local schools to protect them against insufficiently informed conclusions

> from data in the offices of DTA and at the same time maintain a high respect for the objective data that will enable conclusions to rest upon demonstrable facts and not merely sympathy. The site researcher will develop his/her role so as not to become perceived as an advocate of any constituency with the...project.
> (J. Davis case study, April 1978, p. 213)

This was interpreted to mean that the SR would become involved enough so as to be able to empathize with site actors and their concerns, but not so much as to become partisan of any given position. Certain safeguards were established to help them avoid the latter. For instance, they were instructed not to provide feedback to site people. This function would be performed by Research Coordinators, who were clearly removed from the sphere of action.

Countervailing forces included two that were the function of the SR hiring process. One of these was the priority in hiring SR's who would be acceptable to the sites, and the sites' large role in the hiring--at one site the local people advertised for the documentor on their own before DTA was ready to begin the hiring process. A second was DTA's interest in hiring individuals who would be sympathetic to its own ideology--committed, action-oriented people.

It is not surprising, then, that from the very beginning the SR's should take their responsibility to the sites very seriously, or, as their reflections on the first SR conference in July 1975 demonstrate, that a major concern would be the question of ethics. It was clear at that point that DTA had succeeded in hiring the kinds of site researchers it wanted.

It is also clear that, as the project evolved over the next two years, the site researchers, in general, became more invested in their sites. All the changes in project personnel, duties and priorities--and the resulting frustrations--did not serve to counterbalance the fact that the daily reality

for the site researchers in the DTA project was in the field, in their interactions with the people whose activities they were studying. As one site researcher stated in an interview:

> I generally dealt with DTA demands as I had dealt with my parents when I was a child. I did what I felt was reasonable and ignored what I couldn't reasonably do...what was going on at the school was my daily life.

This is not to say that the site researchers were actually coopted by the sites, but rather that they were more invested in the sites than in the DTA project as a whole. Because of this, and because many of the intra-DTA relationships did not work out as planned, the site researchers were virtually left on their own to work out their relationships with the sites. These relationships varied depending on the personalities and priorities of both researchers and site people.

Building relationships. From the beginning, the site people varied considerably in their feelings about the documentation. For example:

- Some TAG's welcomed the documentation, while others resisted it.
- Some TAG's and the schools with which they were working were at odds with each other over a variety of issues.
- Some TAG's and some schools tried to coopt the site researchers, while others had suspicions that they were being coopted by rival segments of the TAG-school project.
- Some site people were suspicious of the documentors because of the advocacy, alternative schooling stance associated with CNS, and others feared an educational establishment insensitivity to minorities' concerns because of the academic reputation of CEPM.
- Some site people were afraid of subjective bias by qualitative research, and others that not enough of the whole, informal story was being told.

In addition, a number of the site researchers found themselves in situations where they felt that site people were trying to use them, to direct their research, or to control the flow of information, or that they

didn't respect them as professionals. Several of the site researchers had come to terms with the fact that the TAG's they were documenting were either incompatible with their own philosophies or were employing strategies they felt were not entirely ethical. A few site researchers found themselves faced with documenting what they saw as failures.

To experienced researchers not laboring under the constant pressures of externally imposed deadlines and growing workloads, many of these phenomena would have been viewed as welcome data. The site researchers in DTA were not anthropologists or sociologists doing their own research for an academic audience. They could better be characterized as hirelings with a hierarchy of masters. These masters included site people, research coordinators, technical assistors, DTA management, NIE, and the educational research community. The information yielded from the relationship they established was key to the success of a long project in which each of these "employers" had an interest.

The site/site researcher relationships were negotiated in context of the vagaries cited above and in the crossfire--either perceived or real--of pressures exerted by this hierarchy. It is little wonder that the project had to deal with a series of issues that grew out of these relationships. Some examples will underscore the importance that they had to the research.

Some examples. In their original conceptualization the contractors wanted to insure what they referred to as both an "inside and an outside perspective." This would provide some control for subjective bias and at the same time insure getting informal data. This was also the major impulse behind the proposed inside researcher-advisor panel that never materialized. As it turned out, the site researchers, left pretty much alone in the sites, had to be constantly vigilant to the dangers of getting so close that they missed the forest or so distant as to miss the depth and richness of the project.

In one of the New York sites, the site researcher found herself in a school where the principal had a reputation for both being enterprising and for "running a tight ship." The two established a cordial relationship that produced a great deal of information. When she submitted the first parts of her original case study, it became apparent that the story very much reflected the principal's perspective. Not only was the TAG piqued that their "story" had not been told, but when the principal's "kitchen cabinet" saw the emphasis on the principal in the first draft, they read it as inaccurately depicting him as manipulative. This forced the principal, in turn, to back down and deny the accuracy of the account. This was one of a series of events that led to a serious, albeit temporary, rupture in site-project relationships that required great effort to mend.

In one of the West Coast sites, where the TAG and the schools being served came to loggerheads over several issues; the site researcher, whose background affiliation was with the TAG found himself in such a sensitive position that he was reluctant to report any controversial information. One result of this was that very little of the real flavor of what was happening came through his notes and analyses.

At a second New York site, the site researcher, a young female minority graduate student, found herself documenting a project that was directed by a group of highly placed businessmen. She found herself having more access problems than most of the site researchers for several reasons. To begin with, the TAG and some school people did not consider it appropriate for her to observe meetings involving sensitive negotiations. They also were "extremely busy" and occasionally forgot to inform her of planned events (their offices were not the kind of place where one could simply "hang out").

In an interview, a consultant for this group made it clear that they simply did not consider the documentation effort a priority because it was not perceived as being particularly helpful to them. These limitations imposed upon the research by the site, and the fact that the backgrounds of the site researcher and site people were so different, caused the researcher to focus her documentation on the more formal aspects of the TAG and the schools, where she felt more comfortable.

Several of the site researchers had to deal with the problem of documenting sites that were fraught with dissension, that failed to do what they had set out to do, or both. This led to a real problem for site researchers, who felt some pressure to depict the sites positively. This pressure had, at least, three sources.

First, conventional project wisdom held that NIE considered these sites successful or would not have continued to fund them. Second, it was known that NIE and DTA policy called for local project directors to see the final case studies. (While this did not give project directors the right of approval or veto, they could, and on occasion did, make things uncomfortable for site researchers when they disagreed with their conclusions.) Finally, there was real agitation to get information that could be used by members of the technical assistance team in school improvement programs. Again, the conventional wisdom was that the technical assistors and their user schools didn't want to hear stories of failure.

The site researchers handled the "documentation of failure" in

various ways. In one case, the original site researcher quit after two months, in part because:

> He could not stand the non-interventionist role required for our site researchers...he couldn't bear to watch the people make what he considered wrong moves. Also he found some of the people personally abrasive-especially when his repertoire of reactions was circumscribed by his role of site researcher. (S. Wilson, Reflections on site researcher hiring and orientation process, August 13, 1975, p. 2).

Another site saw a succession of three site researchers, with none of them submitting a completed case study. The changes in site researcher and failure to deliver a product were both, to some extent, the consequences of a failure by the TAG to meet its expectations and the nature of the site researchers' relationships to the TAG. All three had been "handpicked" by the TAG and all had some prior connection with it. The problems faced by the site researchers, then, included a conflict between personal loyalties and the requirements of balanced reporting.

Two of the site researchers found themselves documenting sites where the direction of the NIE-funded project changed during the research. Both were difficult experiences for the projects involved and created problems for the site researchers as well. In one case, the change came while the site researcher was at a critical point in his writing and analysis. Feeling that the new developments merited his attention, he laid aside his analysis and spent his time on site. Not only did the delay this caused have serious repercussions for him, but he found little positive value to report in the project hassling, and confessed an inability to make use of the negative data he confronted.

In the other situation, the site researcher had something of a reputation as a political activist before taking the documentation job. The original TAG, a state teachers' association, ran into problems in its relationship with the target school. From the beginning the TAG saw the site researcher's sympathies as lying with the school. When the rupture became final, the TAG contract was given to a consortium of the teachers themselves. The original project director, who resisted this move although it had been planned from the beginning, suspected that the site researcher had a role in the affair. She was so incensed at his reporting that she threatened legal action, to have what she saw as inaccuracies redressed.

Summary. Not all of the site-site researcher relationships were as difficult as these scattered incidents might suggest. For the most part and for most of the time, they were cordial and rewarding. However, the role of site researchers, defined by DTA as "non-participant observer," was subject to a variety of interpretations, and the process of building relationships proved complex and sensitive. Given the mix of relatively inexperienced, isolated site researchers, the lack of a clearly articulated research design, a plethora of conflicting pressures, and what were seen as less than exemplary TAG projects to document, site researchers found the road to maintaining fruitful site relationships filled with challenge.

Chapter Summary

In this section we have tried to highlight those characteristics of the DTA project that we consider to be of prime importance in understanding the research effort. Because the project covered a period of six years (two years for the SR's), and because it was conceptualized by NIE as a evolutionary project, we have organized this chapter around the changes that took place and the major decisions that influenced those changes. The five foci are: organizational changes that affected the field research; the evolution of the research design; the unfolding site researcher experience; the evolution of the site researcher's job; and the site-site researcher relationships.

The documentation effort of the DTA project was originally designed to lead to the development of grounded theory--an intense process of iterated data-gathering and analysis. The lack of advance intellectual structure to the research design was replaced by a variety of procedural plans and organizational arrangements of people to carry on the dialogue inherent in grounded theory development. At first, these were to involve an inside-outside perspective, with site people collaborating actively in the research effort. For various reasons, this plan never materialized. Two other kinds of relationships were designed to provide structure to the conversations: (1) the active participation of site advisors--experienced persons who would work intensively with site researchers to help them sharpen their focus as the research progressed, and, (2) the relationship between site

researchers and research coordinators--more senior individuals who would have distance from the site, who would act as liaisons between the site researchers and the larger projects, and who would have primary responsibility for cross-site analysis in interaction with one another. Again, for the reasons discussed earlier, the site advisor role never developed as planned. Site researchers and their research coordinators worked alone, and, in five of the nine sites, were housed in different locales from the funded sites. Partly because these individuals were charged with so many responsibilities, these role relationships were never totally adapted to the realities of the project.

Historical factors also led to a radical change in the orientation of the project by the end of the second year. The DTA ideological commitment to action research and alternative self-perception had resulted in weighing certain criteria over others in the hiring of site researchers and in valuing hoped for collaboration in the research with the nine funded sites. For these reasons the site researchers who were hired were strong on ideological commitment and experience with schools, but weak in research skills and experience. These decisions were consonant with NIE requirements as perceived by DTA. The primary goal of the research was to collect information and present it in a way that would be useful to other practitioners interested in moving in related directions. This knowledge utilization process was intended to be ongoing and not necessarily to be based on the familiar scholarly written products. But, as the technical assistance and documentation aspects of the project diverged, mechanisms for that kind of knowledge

utilization were never successfully developed, and the only products that could be shown NIE were case studies and cross-site papers from the research team. New research coordinators, who lacked the initial ideological commitments of the project, but who were strong in academic credentials, were hired to monitor these tasks. Coming on so late, these research coordinators were never able to understand the sites as well as had been originally intended, so they focused on helping site researchers to write "quality" pieces in familiar formats. The site researchers, originally hired to serve as a sort of conduit of information that would be used by others, were now largely responsible for this product, a responsibility for which none were prepared.

A focus on problems does not mean that the research experience was negative for the participants. In preparation for writing this paper, all of the site researchers were interviewed. Even after reiterating the frustrations they experienced in writing their case studies, dealing with the changes in duties and personnel, and coping with the other vicissitudes of the DTA project, seven of the nine characterized the experience as rewarding. They all felt that they had learned a great deal about doing research, working with people, and writing--among other things.

Some of this learning stemmed from negative experience--in learning to cope with difficult situations at the site and with the project, for example. We have taken a similar stance in writing this paper. Recognizing that the value of any experience can be measured by what can be learned from it, the DTA project has provided us with rich data for explicating research issues--both through its mistakes and its triumps.

CHAPTER 2: SOME CONSTRAINING FEATURES OF THE DTA PROJECT

INTRODUCTION

Six characteristics of the DTA project emerge as having exercised significant constraint on its research effort and as important to understanding the field research issues it encountered. They are:

1. Its non-academic context.
2. The contractual imperatives; what NIE wanted and was willing to settle for.
3. The CNS-CEPM marriage.
4. The underlying ideology.
5. The geographical dispersion of the project.
6. The role of the data management system.

In this chapter each of these program features is briefly sketched as context necessary to understanding the DTA field experience sketched in the preceding chapter and the analysis of the issues contained in the following chapter. A complete description of each of these features and their effect on the project could in itself be an entire paper (indeed, a number of other DTA papers have either been or are being written dealing in detail with these topics.) We have limited our attention to aspects that directly affected the field research effort.

The Non-academic Context of DTA

The non-academic base, location for the DTA research is not particularly remarkable except that two social science disciplines were repeatedly credited as genetrices for its theoretical basis and methodology. DTA documents frequently refer to the research as ethnographic and credit anthropology with some of its perspective (Cf., Response to Questions Regarding DTA's Progress and Plans, Nov. 6, 1975. pp. 139-141). As a guiding theoretical principle field research staff professed to follow the grounded theory approach of Glazer and Strauss (1967), two qualitative sociologists.

It therefore becomes imperative that we understand something of the meaning of field research in anthropology and qualitative sociology if the issues raised here are to make sense. The contrasts between the role of field research in these two traditional academic disciplines and that required of it in the contracted DTA research are striking.

Both anthropology and sociology as disciplines are specific perspectives for looking at behavioral phenomena which have been shaped over long years of debate and reflection. Educational research, in this sense, is not a discipline. The disciplinary perspective of anthroplogy or sociology leads a researcher in other areas to formulate research questions which require kinds of data. Field research is one technique available for getting these data. The research questions posed, the kinds of data needed, the specific contex and the "given" disciplinary perspective determine the qualities required of the researcher and the kinds of field work to be done.

To speak of field research as if it could stand alone, as if it were a self-contained entity or a packaged methodology useable by anyone in any context, is a serious mistake.

This does not mean of course, that it isn't a useful way to study educational phenomena. It does mean that if the kind of field research done by anthropologists is going to be usefully adapted, anthropological like research questions will need to be posed and people conversant with the perspective of anthropology--its holism, its requirement for participant-observation and its use of culture as an heuristic construct--will need to do it. It means that if field research is going to be used as some sociologists use it, for the discovery of grounded theory, users must have a clear notion of the very particular meaning to theory, comparative analysis, conceptual categories and other concepts ensconced in this tradition. Prior orientation to the approach and reflection upon it are especially imperative if the goal is to develop grounded theory, since that requires a sophisticated, self-conscious process of ongoing analysis and refinement by the field researcher.

There are other important differences between the role of field research in traditional social science disciplines and educational research under large contracts. Anthropological research, and to a lesser extent, qualitative sociology has typically been conducted by single highly trained researchers. They may be part of a larger team, they may share findings, problems, insights and procedures with others but they ultimately have the responsibility for the myriad decisions they must make in the field.

For both anthropologists and sociologists the audiences are usually academics who have no vital stake in the findings. Debate about them may be emotional and vociferous but it remains academic.

None of this suggests that any techniques of field research, including ethnography or efforts at discovering grounded theory are to be avoided in educational research. It is to suggest that in their application care be

given to what is required to translate them into alien settings and that the requirements for their effective use are respected (i.e., the particularly demanding skills required of ethnographers or grasp of the definition of theory basic to discovering grounded theory.)

Even those cases where anthropologists have "successfully" researched American education differ, in most instances, from DTA-like projects. These have typically been descriptions of programs with ethnographies as outcomes, have been conducted to further the academic interests of the researcher and have been relatively small scale enterprises of either a single researcher or a senior researcher with a team of tightly knit assistants. (Cf., Smith, Wolcott, Eddy, Spradley, Henry, etc.)

In this effort we are cognizant that DTA is not totally unique. Other projects have used field research techniques borrowed or adapted from anthropology and qualitative sociology. Their experiences, for the most part, underline the importance of raising and dealing with the issues addressed here.

The Contractual Imperatives: What NIE Wanted and Was Willing to Settle for

In 1973 NIE announced a grant competition for support of projects aiming at organizational change in urban schools across the United States. Any school district or other organization could submit a proposal for continuing already existing organizational strategies that seemed to be promising ways of developing something NIE called schools' "problem-solving

capacities." The announcement read, in part:

> The National Institute of Education (NIE) is interested in studying the potential of various organizational arrangements for helping urban schools sustain a process of continuous improvement. The Research Program on Organizational Strategies for School Improvement--of which this Announcement is one part--will (1) provide grants to support the further development or extension of programs for accomplishing the above objective, and (2) contract under a separate solicitation for documentation and evaluation* analysis in order to acquire generalizable information which NIE can use to assist other schools considering similar programs (NIE 1974).

In the summer of 1974, grants were awarded for nine projects, with awards going to school districts, community groups, a community mental health center, a teachers' union, a city university, and several business groups.

In August 1974, another competition began, to select an organization to study these projects and to use the results in experiments helping others. Both the Center for New Schools of Chicago (CNS) and the Center for Educational Policy and Management of the University of Oregon (CEPM) responded to an initial request for statements of Qualifications. According to the DTA Project Overview of 1977, their reasons for submitting proposals for this grant were as follows:

> CNS was established in Chicago in 1971. Based on the prior experience of its key staff in establishing a successful experimental urban public high school, in developing and using qualitative field research methodology for studying schools, and in providing assistance to schools and parent groups, the young organization was considering new ways to join research with providing direct technical assistance to local schools to improve the quality of education for urban students.

*The evaluation language was later dropped from NIE's lexicon and DTA was not contracted to evaluate the projects.

> The Strategies for Organizational Change (SOC) program of CEPM establish in 1967, was seeking new ways to extend its nationally recognized work in applying organizational development strategies to schools. SOC was particularly interested in expanding its activity to include significant work in urban schools (DTA Project Overview 1977, 1).

NIE decided that both CNS and CEPM were qualified to receive the detailed request for proposals and to bid for the 5-year multi-million dollar award. The two groups decided that a collaboration between the two organizations could best carry out the project NIE wanted. Accordingly, the final proposal was a joint effort to address the scope of work outlined in the RFP.

This scope of work provides a useful starting point for our discussion:

> The purpose of the work to be performed under this contract is to study the effects of particular organizational strategies on the capacities of schools to solve their problems, and to develop means of helping others to learn from and modify those organizational strategies to meet their own needs.
>
> The principal, but not exclusive, set of strategies to be studied are those being employed by nine organizations awarded three-year grants in June 1974, by the Program on Local Problem-Solving of the National Institute of Education...
>
> The study is to describe the various strategies in detail, and to gauge their impact on the target schools. The initial questions are: what happened in the course of attempting these strategies, and were the schools better as a result of the strategy or parts of it? The Contractor must also take the information gathered from each site, and any multi-site comparisons or contrasts that seem useful, and use that body of experience to help other schools who are, or could be, interested in improving their own problem-solving...
>
> It should be stressed that the principal audience for the study is the practitioner rather than the researcher. Data, analyses, and forms of communication or help will be chosen for their practical utility in schools...(NIE, Scope of Work, p. 7).

From this it is clear that NIE was offering to fund a project that was in part outside the traditional mold of educational research. It apparently wanted descriptions and evaluations--traditional enough--but wanted them designed to aid practitioners. Throughout the tenure of the entire project the project officer and staff of the local problem solving group of NIE evidenced a willingness to work with the contractors on the experiment.

The projects in the schools started in September 1974, and had in most cases extensive prior history. The study contract was signed in December 1974, so the winner was already some months behind the action. The schedule included in the CNS/CEPM proposal which NIE accepted included a first phase from January 1975 through September 1976, or the first two years of the school projects (1974-75 and 75-76). The final request for proposals was most specific concerning the work of these twenty one months.

Six required tasks were:

1. Documentation of problem solving program at each site.*
2. Cross-site analysis of specific topics or issues.
3. Selection of uses sites and/or ongoing technical assistance programs and within these experimenting with ways of using research information and technical assistance to help schools.
4. Assessment of the utility and effectiveness of technical assistance and materials in aiding user schools in improving problem solving.
5. Preparation of working papers and consultation with NIE local Problem Solving staff to draw implications from the study of the funded sites and related information use tasks for other components of the Problem Solving Program.
6. Maintenance of a log of contractor's activities that will be analyzed in various ways to keep NIE staff informed of ongoing progress and will allow the contractor to document the ways in which various problems were addressed.

*The contract called for delivery of case studies of each site after the end of the third year of NIE site support, and the history of this requirement is an important thread throughout the whole project history.

While a number of specific deliverables were called for in the RFP, chiefly more detailed ideas and plans for the first four key tasks, their content was largely left open. Emphasis was on the contractor conceptualizing and beginning to perform the tasks. A second phase would cover the last year of research in the nine sites, 1976-77. Then a third and final phase would involve the contractor chiefly in further assistance work for two years 1977-78 and 1978-79. Plans were left open for these later phases to be developed out of experience in the early years, although the original contract scope of work did sketch the later phases like this:

> ...important shifts in emphasis among the tasks, changes in research techniques and substantial improvement in apporaches to helping other schools benefit from the experience of the grantees. In particular it is anticipated that the user school group will be larger and more diverse than in Phase I, and that the contractor will devote a larger share of resources to interaction with users in Phase II than Phase I. (Scope of work, p.9)

In both the RFP and the response a timeline indicating key milestones and due dates for Phase I deliverables was established. Unanticipated delays in hiring DTA staff, in getting the organization operating smoothly, in setting up the data management system and in securing agreements with the nine documentation sites resulted in a number of major changes in these original plans, some of which had far reaching effects on the project that are treated in the following chapter.

In addition to the important effect these early delays had in the final shape of the program, they were occasions for the project to discover what NIE really wanted, and more importantly, what it was willing to settle for. Example, questions were raised about moving ahead on the technical assistance before the results of the research were in. This issue culminated in an October 1975 visit by the Principal Investigator from CNS to Washington

to brief NIE on the progress of the project to that date. His message was: site researchers had been hired but were not very far along in thier work, the data management system was still being developed so virtually no analyzable results of the documentation were available, the plan for developing cross-site foci had not gotten off the ground at all, the plans for choosing sure sites for technical assistance as called for in task three were still embryonic and the project organization and staffing plan was still in serious flux.

The Project Officer responded to this situation with a long letter to the principal investigator outlining what he expected. In effect he indicated that the Institute understood, was sympathetic to and accepted as valid the reasons for the project's delays and changes. He expressed a willingness to renegotiate milestones and deliverable dates but sketched certain parameters within which the project must operate.

First, NIE stressed the importance of making sure that a means be found to get information and insights out of the hands of the SR's and into the hands of others in the project. Cross site foci and technical assistant plans should be directly informed by the research effort. In his words:

> We are, in general, concerned that Task 1 and 2, in whatever stage of development, be a far more active force than at present in informing Task 3 plans and designs. We believe there is a lot of cross site substance in the heads of the research coordinators that could profitably be written down as aids to the Task 3 work without waiting for the computer system to begin functioning... (Letter from Project Officer to Principal Investigator, October 9, 1975, P. 3).

Second, the PO while in effect reiterating the exploratory nature of the project and the concomitant flexibility this entailed, insisted that comprehensive plans for accomplishing the task be formulated and adhered to.

This was seen as crucial since the original plans called for the development of a structure and a set of evolving procedures for accomplishing the task rather than simply the mechanism of a set of objectives.

> We are concerned that even granting certain delays in parts of the contract work, after nine months there does not exist an agreed-upon comprehensive document outlining the background, assumptions, rationale and planned actions for any of the three tasks--research, analysis, and utilization. Because of the reasonable arguments you have presented in the past for redefining certain elements of required reports, or changing dates of others, our concern is simply at this time to establish a firm set of agreements about what needs completing and when. (Letter from Project Officer to Principal Investigator, 10/9/75. P. 3).

A final parameter established by NIE within which DTA should operate was that the project keep itself informed and draw upon the experience of researchers, practitioners and interventionists outside of the two contracting centers.

In the words of the PO:

> It is of great importance to the NIE that the work of contract be "placed" in a context of other work in related areas, and that similarities, differences, and (we hope) substantial advances on the state of various arts, can be clearly noted in plans. (Ibid, p.3)

In sum, NIE wanted DTA to experiment with techniques to draw from field research to aid urban schools. The main objective of the assistance, in keeping with the purported goals of the projects being studied, was to be the development of a "capacity to solve problems." The content of these techniques as well as the definitions of "capacity" and "problems" was left unspecified. The only constraints were that any intervention plans be informed directly by the research efforts of the project, that their rationale, assumptions and activities be spelled out and that their relationship to other similar efforts be understood and documented.

One other development that had later ramifications occurred at this time, i.e., the fall of 1975. Because of technical details of the Congressional appropriations process, NIE projects lasting longer than one year must obtain annual infusions of money, rather than getting the whole price at the outset--a process called incremental funding. The first funds ran out in September 1975, and NIE took the re-funding point as an opportunity both to ask some serious questions to the contract term about progress to date, and to have the resulting document reviewed by some non-NIE outsiders--in this case two researchers (on grounds that the plans and work to date involved chiefly the research or "documentation" task of the contract.) Although project personnel were not fully aware of the significance of this review utilizing outside scholars, it meant, in effect, that they could not depend as heavily upon the set of informal relationships and easy rapport they had established with the Local Problem Solving staff at NIE as a guide to their decisions. These decisions, as well as the format and contents of their deliverables, would have to pass muster before persons less intimately involved in the daily vissicitudes of the project and coming to it with varying perspectives.

These interactions with NIE witness that the Institute sincerely wanted an experimental project, was willing to accept the risks this approach entailed and was willing to settle, within the broad parameters outlined above, for whatever this effort produced, so long as it would stand up to critical review. Given the underlying ideology of the project and problems in the CNS-CEPM merger, sketched below, this relationship proved to be an important factor in shaping the field research experience.

The CNS-CEPM Collaboration

At first blush a marriage between these two centers seems strange. CNS saw itself as oriented toward "action research". It had deliberately avoided establishing any close university affiliation and prided itself on being outside the established research traditions. Its major accomplishments had been in creating one alternative school, and thinking and writing about issues in that area.

The CEPM team, on the other hand, was a university-based research and development group with an international reputation in advancing the theory and in the application of organizational development as a means to school improvement. In working style, depth of and nature of experience and ideology the two centers were strikingly dissimilar.

The merger was decided on because both the centers and NIE felt that the two shared a general philosophical commitment to action research which was congruent with the requirements of the RFP. The characteristics of the organizations that were different were seen as complementary and as potential assets in carrying out the project. These characteristics included:

1. Their geographical locations (CNS in Chicago and CEPM in Eugene, Oregon) which meant a somewhat closer location of supervisory staff to East and West Coast study sites and later assistance sites.

2. Different methods for effecting change: CNS traditionally had approached systematic change in urban schools primarily through organizing parents and communities, or through community-based learning options. In contrast, CEPM had focused more on helping schools. School professionals strengthen their group processes so that they could more effectively solve their own problems and rationally adopt, adapt or reject instrutional innovations.

3. Different theoretical approaches to their work: "While CNS has maintained a strong empirical thrust, gathering wide experience with a range of critical activities and features of school organizations that have effects, CEPM has taken a larger portion of time sketching out theory as a guide to school consultants." (Response to RFP, p. 3, para phrase)

In the groups' joint proposal an organizational structure for the project was worked out with co-directors overall, and with assigned tasks to teams made up of people from both centers. The staffs were almost to be merged, with an assumption of constant communication and input into the overall project devision-making process by the total senior staff. Working out the actual shape of this relationship became a matter of prime concern throughout the entire tenure of the project.

While legally CNS was the prime contractor and CEPM a subcontractor, in practice, and in keeping with the CNS ideology, an attempt was made to build a relationship of equality. The research and technical assistance tasks both had co-leaders, one from each group. Supervisors of site researchers, called research coordinators, were drawn from each group. Each was to have a technical assistance site. Programmatic decisions were to be made jointly and agreement was to have been reached on each substantive issue.

This collaborative arrangement, however, did not work out as planned. For instance, while equivalent in the paper structure, the research co-directors had strong differing views that could not be easily merged into a simple work plan. The CNS representative was young, had come from a field research tradition and became the interpreter for the grounded theory approach. The CEPM representative was an older researcher trained in rigorous social psychology and author of a respected research methods text, who was largely puzzled by the operational meaning of CNS' version of the grounded theory approach. The former took the notion of co-directorship seriously and was reluctant to make unilateral decisions. The CEPM counter-part saw the co-directorship as a paper convenience and considered CNS as the proper place

for research decisions to be made. The discontinuity between the structural relationship and the interpretation of this relationship into actual practice contributed to unending confusion and indecisiveness in the direction of the research.

In addition, problems with communication between the two widely separated centers and with task distribution lead to serious delays in meeting proposed deadlines and were important pressures leading to the organizational changes described in the preceding chapter. Gradually the theoretical orientation of CNS came to characterize the research and most research related activities came to be housed in Chciago. By the end of 1976 it was resolved that completion of the case studies research aspect of the project would be the concern of CNS alone, leaving CEPM free to do research on their own technical assistance, or to mine the site data as seemed best for the ongoing TA. However, by the time this decision was made much of the data collection, determination of cross-site foci and first drafts of the case studies were nearing completion and, consequently, the collaboration problems had already had considerable impact on the research.

The DTA Ideology

The ideology of an individual or group is always difficult to characterize. Individuals with diverse backgrounds and loyalties, formed into sometimes competing subgroups within two organizations, rarely act altogether consistently. Further, this report is based in part on retrospective reconstructions of thoughts and feelings, which are always open to more creative than accurate reconstructions. Even so, some effort at characterization seems needed since definite elements of ideology evidenced themselves fairly consistently throughout the project and seem to

emerge, to the present authors, as important to understanding the research.

Eight research coordinators supervised field workers over the three years of the site studies. Only one was from CEPM with the other seven either housed or hired by CNS. Thus CNS ultimately played a much larger role in the research and its ideology came to most heavily influence the direction of the documentation and its analysis.

The one CEPM research coordinator, however, was responsible for three of the sites for two years. Although nominally co-director of research for the first one and one-half years, he was clearly not comfortable with the CNS interpretation of the grounded theory approach. His interpretation is expressed in an interim report he and his CEPM colleague, the project's co-principal investiagtor, prepared in the first summer when NIE asked about plans for research and cross-site analysis:

> In a more traditional sort of R & D project, we might have presented a list of...hypotheses about local problem solving in schools at the time of proposal preparation. But in DTA, we are deliberately refraining from stating hypotheses ahead of the data. A central guiding principle for us has been that of grounded theory. This means that we are deliberately attempting to remain naive until we acquire sufficient data, systematized by their coding and retrievable under many categories. (Schmuck and Runkel, Interim report on Task 2. August 15, 1975.)

The CNS co-director of research eloquently revealed a glimpse of his ideological stance in a private memo he dictated after a conference planned to train site researchers had turned into an occasion to deal with some serious concerns of the site researchers and little of the planned training was done.

> I had worked carefully with research coordinators to design a system for site researchers to participate in the analytic development of DTA and also in planning. I was surprised...the first day of the workshop when they asked for more time to caucus

without research coordinators. I couldn't understand why they didn't...recognize the adequacy of the pathways of influence that we had built in. They expressed this attitude for several days--asking to have meeting time as a group without research coordinators...

As the week went on, I came to know the site researchers as individuals and to be reassured about why they were asking for what they were...

I realized the danger of paternalism. Even with all our grand designs for site researchers' influence, it was still something we were laying on them rather than something they chose. I could see the importance of them actually "winning" a role for themselves.

I am not quite sure what to do about training. My tendency is to have faith in people's ability to learn what they need to learn...

One is tempted to use the descriptors, "democratic", antiestablishment" and "close to the data" to capture the essence of this ethos. However, even these are too general and carry too much pre-infused meaning to be accurate. A more realistic approach is to pose a set of situations revealing the positions DTA ideology led to:

- In assessing qualifications of personnel for staff positions, for example, schooling, degrees, and research experience were seen as less important than ethnic identity, enthusiam, personal rapport with people working for change.
- In planning studies, research for research sake or to further academic ends was eschewed in favor of direct intervention strategies.
- Insistence on carefully constructed conceptual frameworks to guide observation was shunned for fear that they would lead to observer bias and overlooking important phenomena.
- Top-down formulation and imposition of directives was abandoned in favor of consensuss, with more weight given to the views of those closest to the ground.

One year into the project the Chicago co-director of research wrote to his senior staff on this last point:

> In order to actualize (the) grounded approach, I see the necessity of treating ideas equally--those from (a junior staff) who hasn't been to graduate school but has a lot of experiece in the community the same as those of Phil Runkel who has written...books and...articles. (Memo from The Chicago co-director of research to DTA staff, December 8, 1975, p.5. Parenthetical inserts added.)

In short, three basic components of the DTA research ideology emerge that were crucial in shaping the experience. There was a confident belief that anyone, given adequate support and encouragement, could do the required reserach and analysis whatever their level of formal training. There was a premium placed on getting input from as many individuals as possible and upon reaching consensus in major decision making. There was a conscious attempt to keep the research and the researchers as uncontaminated by either preconceived notions or outside influences as possible.

This idealistic philosphical stance frequently proved problemmatical in the actual research arena and was a major contributor to the shape of the DTA experience. For example, while it led to the hiring of an enthusiastic group of committed Site researchers who showed promise of uncovering important new insights, the inability of the project to offer the kind of support needed by them led to a great deal of frustration, and in some cases, to a paucity of important field data. The insistence on permitting everyone a voice in formulating cross-site foci led to the curious situation in which the foci were chosen by vote from among a large number of possible options. The effects of this ideology were important factors in shaping the field research experience.

Geographical Dispersion of the Project

DTA was a national effort in several senses. The nine funded documentation sites were located in New York City (4), Washington, D.D., Louisville Ky., Minneapolis, Minn., San Jose and Los Angeles, California. The documentation project had offices in New York City, Chicago and Eugene, Oregon. The project itself was monitored from Washington, D.C., and was a major effort of the Local Problem Solving Group at NIE who hoped that the results could suggest serious alternatives to standard ideas about product dissemination as the major route to school improvement. This dispersion had both predictable and unexpected effcts on the field research given the project's perceived mandate and ideology.

The original design recognized the problems posed by the relative isolation of each documentation site; accordingly, plans called for a semi-self-contained effort at each, to consist of a site researcher, a panel of collaborating "insiders" from site staff and clients and a competent site advisor from a nearby university to assist with methodological and theoretical aspects. The main functions of the central organization were to be coordination, storage and processing of data, multi-site analysis, and linkage to technical assistance teams. Success for several key features was, from the beginning seen as depending upon intensive interaction between site researchers and their research coordinators as well as among site researchers.

As the project evolved, communication among the different sites and the central offices became even more important. In the first place site researchers actually hired were in many cases incapable of designing serious, sustained field research on their own or with minimal help. Inside research panels turned out to be complex politically, so they were never established.

In addition, the site advisor role, which was originally conceived as providing major support for site researchers, was scaled down considerably, leaving the research coordinator-site researcher relationship as the primary source of site researcher support and linkage to the project as a whole. Finally, in accordance with the participatory nature of the DTA ideology, value was placed on wide participation in project decision making.

To insure this interaction money was included for extensive travel by senior staff, a series of conferences was planned at crucial stages (one for training site researchers in the summer of 1975, a second for working our cross-site foci in February 1976, and a great many other sessions for research coordinators and senior staff). WATS long distance lines were installed in the New York, Chicago and Eugene offices and a schedule of conference calls was set up. Even so these measures proved seriously inadequate to coordinate the planned tasks among so many actors; the inability to interact as intimately as anticipated became a major reason for changes in the research coordination assignments, with all supervisors centralized in Chicago.

To summarize, the geographical dispersion of the project, given the inadequate coordinating mechanisms established, greatly affected the work of the field researchers. It resulted in their working in relative isolation, in their being denied much of the anticipated support and direction, reduced their input into the decision making mechanisms of the project and impeded the flow of data from the sites into the formulation of cross-site foci and user sites. Much of their interaction with supervisors and colleagues was by telephone and this proved frustratingly inadequate. Research coordinators, members of the technical assistance teams and project supervisors found it difficult to get the kinds of immediate information they

thought they needed.

The Data Management System

On paper the proposed data management system (DMS) looked like a significant breakthrough in educational research. Using a data-entry terminal purchased for another CNS project, the Chicago team thought that voluminous field notes from the nine sites could be sent to a central computer for storage, with index words associated with segments of text for quick retrieval in a form useable to analysts and technical assistors. In fact, the DMS never came close to realizing these hopes. Contrary to expectations, its development consumed huge chunks of time and money, foundered on ideological and technical concerns of the project staff and drastically changed the research plans of the project.

The original plans were elegant. In the original scheme, site researchers were expected simply to add some modest coding or indexing codes to the usual field-note process. In the DTA concept, field notes were to be recorded on pre-printed lined paper (in two-ply carbon sets called FDFs or field data forms), notes to the left, observer asides to the right. Standard codes for people, places and central analytic themes were to be assigned to chunks of texts--some in the field, some by coders in a central location. Then the text and codes would be typed onto a storage cassette through the data-entry terminal in Chicago for later transmittal at high speed to the University of Chicago computer.

Virtually every aspect of the plan proved either impossible or much harder to execute than expected. Site researchers preferred to take field notes on other paper and to transfer them to th FDFs later: it turned out that much was omitted from the transcription because of the tediousness of copying. The codes grew voluminous and site researchers refused (or were

incapable) of using them, leaving more and more central coding to be done. The central coders in turn had inadequate training and supervision and had unending difficulties with even the easiest codes, let alone the thematic codes. These thematic codes proved a bone of contention among senior staff as to their nature and extent. The date-entry terminal malfunctioned repeatedly, to the intense frustration of its owners and operators, and delaying effective retrival. Finally, the programming of the central computer retrieval process required months of consultant effort, in part owing to design requirement that it be possible to add codes repeatedly throughout the project as new analytic themes were developed.

These problems proved to be much more than simply onerous irritations to the process. The disagreements that erupted over the coding system revealed cracks in the fragile structure that united CNS and CEPM which eventually led to important organizational changes. The planned development of a process for formulating cross site foci and for linking up with technical assistors had to be put on a back burner. The analysis which was promised under the provisions of Task 2 had to be put off.

During the entire summer of 1975 the field data were kept on the sites by the site researchers while the DMS was readied, creating a backlog of work for the coders and coordinators to review, and making analysis or assessment of the data by the RCs difficult. The coding which was eventually done was of such questionable quality that the data could never be retrieved with any assurance that they were accurate.

In the fall of 1975 a new approach was tried. Site researchers were not required to do any coding at all and their efforts were turned to more focussed collection of data for the case studies each was to prepare for his

or her own site. Still later the use of FDFs was largely abandoned in favor of less complete and less frequently submitted analytic logs which summarized and analyzed the data from the field notes. Less and less reliance was placed on the DMS. However, the early experiences with it had drastically changed the research process, and in turn this affected the technical assistance.

Chapter Summary

Not only was DTA large, national in scope and federally contracted, providing an obvious contrast to single, shorter or small-group university based efforts, it was also a collaboration between two organizations with very different backgrounds, divergent philosophical and theoretical orientations that were widely separated geographically. Furthermore, it was at times described as an experiment, and as such implied great lattitude for the interplay of ideologies, the development of relationships and the research processes to be developed. All of these features, taken together serve to explain much of the unfolding experience sketched in Chapter I.

CHAPTER 3: SOME ISSUES IN CONTRACTED FIELD RESEARCH

INTRODUCTION

This chapter presents serious problems of conceptualization. It is presented as the denouement of the paper. It is the place where we make sense of the tangled mass of events partially unravelled in the first two chapters, where we present the important lessons to be culled from the experience. Yet, from an experience so complex and rich in many ways, there is the real danger that we will reach profound and exciting conclusions that, were further facts uncovered, would be unwarranted. On the other hand we would like to avoid "reinventing the wheel" or "beating dead horses." These fears notwithstanding, we believe there are important lessons to be extracted from this experience. We leave the question of "which?", "for whom?" and "for what purposes?" open.

This "cop out" does not entirely resolve our dilemmas. We need to write for an audience, to assume a perspective. Since this is written as a contractual obligation to NIE we consciously present issues, as we feel, they must be faced by people who may be making decisions to fund contracted research. We hope they will be of interest and use to practitioners and academics as well, but rely upon their ability to make the necessary translation and application.

We also had to choose a format and an organizational convention.

We have chosen to divide the chapter into two main parts. Part I reviews some of the givens which must be taken into account in doing field research. There are no surprises here. As obvious as they appear, they are occasionally overlooked in designing field research projects. Because

they are important to every issue we have discussed them up front to avoid belaboring the obvious in discussing the issues. Part II presents the major issues wrestled with throughout DTA.

In both sections we have had to face a problem familiar to DTA SRs, i.e., how to analyze negative evidence. Most of the issues presented here were never satisfactorily resolved by the project. In these cases the DTA experience does not provide definite answers. It does, however, reveal some possible approaches to them and does illuminate in stark relief some of the reasons they worked as imperfectly as they did. This is all valuable information.

One other decision has been made with respect to conventions. Throughout Chapter III we have used the term "field researcher" instead of "site researcher." This is done deliberately to articulate a change in emphasis. Up to this point we have described project-specific events. While the issues emerged directly in the analysis of those events, they have been abstracted from the data and represent issues that must be faced by any similar contracted field research effort.

PART I: SOME GIVENS IN FIELD RESEARCH

There are a number of givens which will inexorably shape the research experience whether explicitly addressed or blithely ignored. The four (with corollaries) listed below figured prominatly in the DTA documentation effort either because they were consciously ignored or because of debate over ways to address them.

We have chosen to treat the givens before the issues to avoid repetition. Each affects, in some way, each issue. We have also chosen to treat them cursorily, not because they are unimportant, but because they are predictably important. They would be important to any field research effort and have been amply discussed in the literature.

A Conceptual Framework

Every researcher will operate with a set of cognitive categories and perceptual strategies through which her/his experiencing will be processed. Not to recognize this reality can have serious consequences. This framework can be one inculcated through a program of deliberate training or it can be that which has developed through the life experiences of the individual.

Dealing with this reality was a problem throughout the DTA research. First, the field researchers represented a variety of disciplines. Each came to the task, as a result of her/his background with a different conceptual framework. Second, the project, in effort to avoid losing the richness promised by these differing perspectives, deliberately refrained from indoctrinating the researchers with a single framework.

There is no evidence that this strategy resulted in decreasing observational prejudice but it did deny the researchers the comfort of "knowing" what their bits of data meant and how they related to a larger whole. It made the development of analytic themes and the formulation of cross-site foci extremely difficult.

A Mechanism for Deciding What to Record and What to Ignore

Field researchers were frequently admonished to be sure to record everything. While the impulse behind this advise is understandable, the notion itself is ludicrous. Attempts to do so carry great risks. If field researchers try to record everything they will miss important stuff in the doing and will lose the important stuff they have recorded in the chaff. They must start with some prior mechanism for making these decisions.

A corollary to the fact that field researchers cannot record everything is that the best field notes are not necessarily the most detailed. They should rather be good mnemonics. Their crucial test is, "Are they amenable to the accurate reconstruction of the meaning of events?"

An Audience

Field researchers never operate in a vacuum. They "know" that someone is always looking over their shoulder. They have to write for someone. This is even more of a problem for researchers hired as part of a complex project than it is for academic researchers. Explicit definition of these audiences can go far in anticipating serious problems at future dates.

The Researcher Is The Primary Research Instrument

Utilizing the "investigator as instrument" is the major advantage of participant observation research. Several attendent problems are discussed in this section.

Researchers need training. Researchers are not sent into the field simply to live. They are expected to exercise skills of perception, of reasoning, of filtering, of analysis and of rapport building. These skills are task-specific and seldom come naturally.

Researchers must be flexible. A major value of first-hand observations is that they raise additional questions. Field researchers can do what paper and pencil instruments cannot. They can go off on tangents, adjust in midstream and turn surprises into real data. They must know how to do these things without getting lost.

Researcher perceptions will be limited. Researchers will have their own cognitive screens and can only be one place at a time. They will tend to see "trees" instead of "the forest". Their tasks, roles and support services must be designed with these limitations in mind.

Researchers need to be buffered. Because they are human and because they are on site, field researchers will be used. They will be asked to take sides, they will be dumped on, they will be misunderstood and they will be coopted. Mechanisms, either intrinsic or extrinsic, must be in place to deal with these realities.

Researchers carry the important data in their heads. An important advantage to using field researchers is that the "instruments" themselves have incredible capacity for storing and processing information. This has important implications. It means, for example, that even field notes are at

best first level analyses and that the researchers themselves must have an important role in writing up conclusions.

Researchers' specific skill levels will be uneven. Some field reserachers will be facile writers, some will be good at observation, some will be superb interviewers and some will be brilliant analysts. Few will have all of these skills and none will be equal in all. In a complex project measures will have to be taken to compensate for the weakness.

Researcher, is only one facet of their lives. The importance of this fact cannot be over emphasized. Researchers have families, career aspiration, responsibilities and a host of other demands on their time and attention. They may see their research as peripheral to their lives or as a consuming interest, they may be able to shield it from the rest of their lives or they may find it all entangled. At any rate, their extra-researcher lives are going to affect their research and as sensitive "instruments" its output.

As indicated earlier, each of the above realities must be reckoned with in doing field research. Their implications and ways they can be woven into a project are infinite. The way they were addressed or ignored figured prominantly in the DTA experience sketched above. They must be made fundamental considerations in resolving the issues outlined in the next section.

PART II: SOME ISSUES IN CONTRACTED RESEARCH

Our major criterion for chosing issues has been to focus on those our investigation has demonstrated were of direct concern to field researchers. Even with this guideline we found it necessary to present two

categories. The first, which are labeled "General Research Issues" involve management level decisions directly while the second, "Field Researcher Issues", have as their focus researcher relationships. Some of the second category are specific permutations of the former.

To even label each as single issues is somewhat misleading. They are properly clusters of questions. In each instance the major arguments advanced by CNS-CEPM for the decisions they made in resolving the questions are followed by what we have ascertained, after the fact, are in fact the main considerations to be addressed in making such decisions.

General Issues

Relexification and redefinition, translating social science research concepts into plans for action in educational research. Ferdinand De Sauaaure, the father of modern linguistics, discussed the apparent paradox presented by, what he called, the mutability and the immutability of language. While language inexhorably changes, it cannot be changed by individual fiat. (Dr. Saussure, 1911) The reasons behind the conservative impulse in language are found in the social and psychological functions it performs. Words and concepts become mutually baggage and vehicle for each other. At a practical level, this fact creates problems to either the application of concepts or the adaptation of labels across the disciplary boundaries separating social science from educational research and impinges on the whole question of developing novel tools for educational research out of extant knowledge.

Three terms-concepts figuring importantly in DTA illustrate the issues.

DTA made heavy use of the notions of "grounded Theory" and "ethnography", terms taken from the lexicons of two disciplines, qualitative sociology and anthropology, respectively. A third term, "documentation" which

claims no specific disciplinary genesis, was used to describe the task of the SRs.

The first two concepts were used with the expectation that they could be adapted to the needs of the DTA research effort and could contribute to the generation of an eclectic approach. It was assumed that the connotations they brought with them would give some direction to the project participants and comprehension to outsiders interested in the DTA approach. It was recognized that they would have to be adpated and redefined but it was assumed that they would easily take on new situated meanings. The term "documentation", on the other hand, was chosen precisely beacuse it didn't bring a great deal of baggage. It was used to articulate NIE's intention to neither have DTA evaluate the site projects not to subject them to traditional social science research.

However, given the size and complexity of the project. the absence of a clearly articulated research design and the lack of a single source of articulate, active intellectual direction none of these assumptions or intentions bore up. The traditional connotations of grounded theory and ethnography created confusion for inexperienced researchers or those coming from radically different traditions. Their application to the DTA context generated cognitive dissonance for the SRs grounded in anthropology and sociology. By the same token, the more neutral, "documentation" permitted a variety of interpretations. Rather than being a symbol for a specific well-defined and somewhat novel task description it was seen as a code for evaluation or ethnography or as a bureaucratic obfuscation.

This experience does not suggest that new tools cannot be synthesized from existing pieces of knowledge nor that ecletic approaches are to be avoided. It does suggest several cautions. If terms with heavy situated

meanings attached to them are to be adapted, users must acquire a thorough and profound understanding of those meanings so that necessary modifications can not only be made with an awareness of their many implications, but can be defended. If neutral terms are going to be used they must be carefully defined with explicit attention to the related universe of concepts with which they contrast.

This process can be fruitful in generating new perspectives, approaches, tools and insights and, indeed, may be the way most paradigms are actually adjusted. Engineering it, however, is a best risky and in the arena of action research as complex as they attempted by DTA is fraught with difficulty.

Collaborative research: how to achieve parity? CNS and CEPM, with the blessing of NIE, decided to work together in DTA. In cahpters I and II we reviewed their differences and similarities and the nature of the "contract" between them. The arguments for collaboration were cogent. Both centers shared an underlying ideological commitment to action research. Their approaches were seen as complementary and their locations as giving wider geographical range to what was a national project. "On paper" CNS was the prime contractor with CEPM having a subcontract to them but a strategy was devised giving both centers equal management and decision-making responsibility. Despite these arguments and arrangements, the collaborative relationship was never comfortable for either center and, as we have seen, contributed to a number of DTA's research problems.

With the advantage of hindsight it appears that general ideological compatability and complementarity of strategy, while perhaps necessary conditions for maximally useful collarboratien are not sufficient. A prime

consideration has to be parity in personal investment and expected reward. This parity was absent in the CNS-CEPM marriage.

In supporting this contention it is difficult to avoid unwarranted generalizations since the staffs of both centers represented a variety of positions. However, several facts are clear. For CNS, which was not university affiliated and prided itself in its "nontraditional" approach to schools, DTA was almost a consuming passion. It represented an opportunity to prove something to the educational research establishment. CNS really believed the project team could bring a "humanistic", "democratic" set of principles to bear on some knotty problems facing urban schools and improve them. It felt compelled to demonstrate that urban problems could not necessarily be solved by a top down approach and that universities need not be the only centers for the generation, synthesis, and packaging of knowledge. It further felt there really were some new things to be learned from looking first hand at schools in action.

The reward for CNS would be the kind of satisfaction ideologues receive when they have succeeded despite the odds. It was going to succeed, without compromise to its principles, or become neurotic.

CEPM's investment was different. To it, DTA was an interesting idea, a chance to demonstrate the usefulness of some research and technical assistance strategies it had developed. It was less interested than CNS in making new discoveries that could be applied in novel ways to urban school problems and more interested than their counterparts in finding new information to refine or support an approach they had already developed.

As tenured university faculty the two senior staff in the CEPM group did not have the same stake in the success of the project that senior CNS staff did. Their counsel was to competently go about doing what could be done, within the mandate and given the viscissitudes of the

project, and not spend undue time or emotional involvement in resolving, to everyone's satisfaction, every unanticipated side issue that arose. Their reqard was to have been the satisfactory completion of another project they had already demonstrated competence to do.

The absence of parity in personal investment and in expected reward does not neccessarily imply a quantitive difference in level of effort or of committment to a project's success. It will however, manifest itself in potentially confusing differences of operational style, research focus and demands on field researchers. In DTA a number of the research problems that erupted can be, at least, partially, traced to this lack.

Task or team orientation: weighing the advantages of centralized or diffused structures. The decision as to whether to focus on accomplishing a priori tasks with the project personnel operating as a plenary group or to put the emphasis on team building within the group, is an ephemeral issue involving sets of trade-offs that must be decided sui generis. DTA experimented with three specific models.

In the original model focus was on accomplishing the several tasks defined for the project. While specific individuals, dispersed throughout the project, were assigned primary responsibility for each task, the staff as a whole was to attend to all of them. Later, a research team was constituted with a single leader but the second level structure was dispersed with one of the research co-ordinators having the second role of directing the data analysis. Finally a centralized team was developed with responsibility for data collection and analysis and little concern with application of findings. Each change in structure was instituted to overcome deficiences in it predecessor.

The experience raised both conceptual and operational issues. The conceptual problem is simply, "Are structures of the first type compatible with the givens of field research?" If, for example, the field researcher is, by definition, indispensible to data analysis and writing conclusions, as well as collection, (as we have argued in Part I of this chapter) is it empirically possible to have documentation and analysis treated as separate tasks with different individuals being responsible for each? This specific is raised simply as an example of the larger issue, viz., "Are there some structural models which are incompatible with useful field research?" The DTA experience, while not providing a definitive answer, does suggest this as an area for serious concern.

Along with the question of conceptual compatibility, the original DTA model floundered on operational issues. An approach that diffuses effort to the demands of a variety of tasks requires that every participant have a clear understanding of each task, its fit into the project as a whole and his/her responsibility to the accomplishment of each task. It further requires frequent and open communication between every participant in the project. The success of this approach requires the presence of a more precise comprehensive research design than a more centralized team oriented model does. By the same token, a "design-free" research model that depends on the orchestration of an evolving process can only succeed where there is a central intellectual force with the power to carefully direct every aspect of the research.

To summarize, the original DTA research structure reveals a possible conceptual incompatibility (between it and the definitional requirements for field research) and a demonstrable operational imcompatibility. Insuring both types of compatibility are important concerns in the design of field research projects.

Ideology: maze or road map? Our investigation shows several important general ideological issues that need attention both by further exlicit scholarly attention and in the design of future field research projects.

The implicit DTA argument for adopting its ideological stance (recognizing that the whole notion of a real choice is something of a fiction since participant ideology was what it was) is that any ideology provides a useful basis for research if it is internally consistant and if research goals are not predetermined at a restrictive level of specificity.

Again, the DTA experience neither confirms nor disconfirms these assumptions, but it does flag some of the pitfalls to such an approach. These are best viewed as a series of unanswered questions clustering around three points. Maintaining ideological purity: How many of a project's staff must "buy into" the ideology? What weight should be given to ideological compatibility in staff formation? Is this best assured at hiring or through indoctrination? How much compromise can a project ideology sustain without serious reprecussions?

In the final analysis, of course, each of these questions must be answered in a specific context and the answer will depend on a host of factors. They cannot, however, be avoided and decisions will have far-reaching ramifications. The DTA experience is instructive in this.

The small group who prepared the original proposal and who conceptualized the project were pretty much of one mind ideologically. In adding to the group, care was taken to insure that new staff shared the ideological committments of the two centers. In time, the developments traced in Chapter I forced senior staff to make a number of ideological

comprises and to hire second level staff with less consideration to ideological purity. While these moves facilitated the process of getting out products, they had some disturbing side effects. Field researchers became even more confused about their roles and foci, several conflicts erupted between field researchers and research coordinators and there was a general lowering of morale among the researchers. More, research output was affected. Virtually all efforts were abandoned to experiment with new ways of going from research to practice and the research deliverables took pretty much a traditional format, profiting little from the novel insights anticipated to have been generated from the original ideological stance.

1. Interfacing with other ideologies. How to keep from being labeled as trivial, naive or idealistic when your approach flouts conventional wisdom? How to insure that your efforts are going to be of quality when the traditional standards, available to you and wielded by your critics have evolved in another ideological context? There are two of the questions posed by the DTA experience.

DTA, as a research effort, faced a major paradox. On the one hand CNS specifically rejected the validity of research for research sake. To be valid, findings had to be directly applicable to real life contexts. Thus, the project deliberately assumed an ideological stance at odds with that supporting the establishment it hoped to impact. The dilemmas stemming from this paradox became painfully difficult to deal with.

From the beginning outside project advisors and the project officer at NIE expressed some skepticism and confessed some bewilderment over products and procedures. The project found itself in the increasingly

defensive position of having to put off requests for results with future promises and elaborate arguments for forebearance. The ponderous structures evolving in the application of the project's ethos proved poorly suited to giving the kinds of clear and immediate results desired by these important outsiders. It continually had to counter the growing suspicion that its assumptions were naive and idealistic. It, in turn, became frustrated that it wasn't being given "a fair chance".

When products were finally forthcoming NIE and the outside reviewers, who represented the educational establishment were almost uniformly disappointed. How much of this disappointment is due to their bringing an unwarranted set of standards generated by their ideological underpinnings to the evaluation, and how much of the burden must be borne by the project for either not producing quality or not adequately defending their findings, remains moot.

At any rate these experiences highlight a significant issue. Given a non-traditional ideological stance, how is it possible to make sure (and know) that the research effort, while in progress, is going to lead to something useful and how to make sure that negative results don't result in the "baby being discarded with the bath?"

2. Basic ideological incompatibility. Is it possible that certain ideological perspectives are so fundamentally at odds with the underlying ethos of American education that their partisans can only work outside the educational mainstream?

This possibility is neither as disturbing in implications nor as radical as it may first appear. It is a possibility that must be seriously considered as an alternative approach to American education. It is the

position that has been taken by many alternative school people in the past. Their reasoning goes somewhat as follows.

Urban education is not really the abysmal failure it is so frequently depicted. Schools do what they do beacuse society wants them to. The basic function of schools is not to ensure that everyone achieves his innate learning potential but to make sure students are properly screened. The system works because the "right" ones succeed and the "right" ones fail. Therefore, to attempt to change schools is futile since any effort will run counter to powerful impulses in society as a whole. The only workable strategy is to establish alternatives that will directly benefit students enrolled in them and, hopefully, exert indirect pressure to bring about changes in mainstream educational institutions.

DTA confronted this position head on by attempting to directly impact the educational establishment through ongoing technical assistance while operating with an alternative ideology. Its success appears to have been minimal. Successful resolution to the several sub-issues discussed in this section may have increased this impact. The verdict is not unequivocal but the issues demand further attention.

The art of supervision: flexilility without ambiguity and quality without stifling control. The intrinsic demands of field research are not easily compatible with outside supervision. The traditional apprentice pattern in academia is a one on one relationship between a professor and a graduate student who has reached the status of junior colleague. Successfully supervising a field research project of the size, complexity, mix of personalities, representative collection of disciplinary perspectives and constraining project features characteristic of DTA presents staggering challenges. The basic dilemma was, how to insure the integrity of the field research approach at the same time assure a satisfactory return for the

sizable investment of money, time and political capital. DTA's researchers were not just hired to get interesting and potentially useful information. They were hired and paid to produce information that would be accurate and useful for the immediate improvement of urban schools.

DTA's solution to the problem of providing the flexibility demanded of field research and the assurance of quality results expected by the contract was to develop a "professional" operating model. Field researchers were viewed, more in the beginning than at the end of the project, as semi-autonomous professionals. This meant that they were left a major responsibility in making the decision as to how to meet the information requirements of the project. To facilitate this every effort was made to keep them fully informed of project developments at every level. Furthermore, field researchers were given freedom to use as they saw fit available research resources. For example, they decided who to hire as advisors and how to use them. This approach had several interesting side results.

The decisions to keep field researchers fully informed so that they could decide how to meet project needs was helpful to the experienced researchers who knew pretty much what they were going to be looking for anyway. However, it created problems for others. They found themselves innundated with paper and had no way to establish priorities in the requests they were receiving. They tended to treat as equal importance a request to write reflections on a meeting, their RC's suggestion for changing the organization of a report and a memo asking for input on a top management level decision which they knew little about. On several sites this beca. : a serious problem contributing to the researcher getting behind and producing thin field data.

The rational for treating everyone as "arrived professionals" rather than seeking a lower common denominator appears to be the belief that people will rise to expectations. In the context described here, however, treating non-professionals as professionals can create serious burdens for the researcher. Concerns to fulfill their supervisors' expectations can constrain their willingness to admit confusion or ignorance and to ask for help. This may be a good way to force growth but the pain it engenders can be excruciating for both the grower and the supervisors who are left to pick up the slack in the research.

Finally, there are several models of what constitutes a "professional". People coming from an academic social science tradition felt that professionals should be truly left alone to make their own decisions, do their own things and take the consequences. Researchers who came from an educational background felt that "the lesson plan" approach did not confront their sense of professionalism. In fact, for these latter, not to give clear direction was a failure to respect their professionalism since it left them with no clear notion as to how they were going to be judged. For researchers of this ilk what the project saw as flexibility was regarded as frustrating ambiguity.

How soon can information realistically be gotten from field research into practice? This issue, which plagued DTA throughout, is a sub question to the whole problem of knowledge utilization and adaptation which is the subject of a detailed analysis being done by Meara and Miles as a separate DTA papers.

The original DTA design called for the indentification for user schools, a plan for linkage between them and the documentation effort and the

identification of cross site analytic themes in the first year of operation. Two developments hindered the process. Field researchers found it difficult or were reluctant to supply information at the early date called for by the proposal (about two months after they had entered the field) and what they did provide was not seen as useful by the user sites.

The reasons, reflect in part, the perennial problem of researcher penchant for dealing in abstract general themes and school people wanting answers to immediately pressing problems. Field researchers need to spend a lot of time in the beginning building relationships and working out the perspective they are to take. These are both sensitive problems and can only be resolved through processing lots of information. This takes time. Any conclusions reached during this stage are seen as tentative and to be guarded lest their divulgence unfairly present the positions of participants.

This is only part of the issue. Even when precautions are taken to insure that the transmitted information is recieved as tentative and that individual reputations are carefully safeguarded, information at these early stages is, of necessity, so site-specific that its chances of being useful to the equally site specific needs of the user are slight.

The underlying, and still open question is, "how much of this perceived necessity for processing information--abstracting, analyzing and translating--is intrinsic to the nature of knowledge and human processing apparatus, and how much an artifact of our academically generated mechanisms research superstructure?"

Data management: (how) can qualitative field data be computer stored and processed for effective use by practitioners? Again, while this is an important issue raised and wrestled with by DTA, the data are too fragmented and comprised by unrelated operational problems to be fruitfully analyzed in this paper. Several other DTA papers deal with it in some detail and,

with the exception of a single passing comment, will not be treated here.

The coding system, essential to computerizing data, engendered a great amount of confusion and controversy. Partisans of the various ideological perspectives in the project were concerned that the codes accurately reflected these perspectives. Despite these concerns and the ultimate compromises that were reached, field researchers had a great deal of trouble with them. They found them hard to "buy into" whole heartedly. The reason is clear. First, while all field research is categorized into some kind of de facto, if not a priori, codes, these are typically those the researcher has either devised or adapted him/herself. This was not the case in DTA. Second, all categories used by field researchers are at first tentative and never, no matter how frequently refined, "God's truth" final. The DTA research data had to be immediately parsed for coding and the researchers feared that tentative decisions would become reified.

At least part of this latter concern was more perceived than real. It is, we are told, conceptually possible to develop a coding system that is both neutral enough and has built in adjustment mechanizms to obviate this problem. Nevertheless, in such a program steps have to be taked to insure ownership of the coding system by field researchers and to allay their fears that the real tentativeness of their conclusions is not ignored.

Field Researcher Issues

The focus of the investigation reported in this paper was the field researchers' experiences. Most of our interview time was devoted to talking with them and in our document perusal we gave particular heed to correspondence from or to them. Virtulally all of the dicussion and analysis to this point has reflected this approach and a logical conclusion to the paper is a disucssion of those issues most directly involving the field

researcher. In the process of developing the context for explicating these issues we have presented here as summary statement with examples where appropriate, of issues arising in the establishment of relationships between field researchers and the unavoidable relationships intrinsic to a DTA-like project.

Of significance to all three sets of relational issues, is the prime reality that field researchers, no matter how carefully supported, buffered or supervised, find themselves essentially alone in the field. They, themselves, must decide how to build rapport, how to present themselves, what roles to assume on the participant--observer continuum, who to talk with and what to examine. Role descriptors like documentor, evaluator, ethnographer or non-participant-observer--those used in DTA--are simply labels until filtered through the cognitive screens of the actors and played out in real life.

In DTA nine different sets of perceptions, values and beliefs were brought to the field by the researchers. The emerging array of experiences is rich. In this analysis we do not attempt to present all, or even most, of this richness. We are concerned to uncover patterns that make sense of the totality and to focus on specific examples that speak to issues of general importance. Furthermore, this being an issues paper, rather than one that "tells the whole story of DTA" examples have not been depicted to meet the requirements if the role purpose were to "fairly" tell that story. In several cases counter illustrations could be added.

Researcher-task relationships. Several dimensions of the research--task relationship are important to doing successful contracted research. Among them, the DTA experience illuminates two which inevitably loom as problem-

matic. They are definition of the research problem and researcher role, and the kind and degree of investment in the task. The first of these emerges directly from our analysis of DTA while the second has become a universal concern in non-academic research and explicating it was a major impulse in the design of the present project.

1. Definition of Research Problem and of Researcher Role. In DTA, as with most contracted research efforts, field researchers were hired to investigate a pre-defined research problem and to fill a pre-defined role. The DTA question was, "How do these nine technical assistance groups help schools develop problem solving capacity?" Answering this required coming to understand both the workings of the TAG and how they interacted with the schools they were supposedly helping. The researchers were constrained to arrive at this understanding from the perspective of non-participant observer.

The question itself raised an immediate problem for some of the researchers. In several cases, the TAGs didn't appear to be helping the schools. What does a field researcher do then? The DTA researchers found a number of solutions, each reflecting the experience and training she or he brought to the job.

One spent all of his time with the TAG reasoning that he could at least write about its internal political maneuvering as useful information.

One focused her energies on the school where a lot of other interesting things were happening and took the stance that even though the TAG had become simply a conduit for providing money to keep the school open, the project was a success because it produced such interesting schools.

A third spent his time at the school, frustrated because he didn't feel he had a focus. In his words, "I simply plopped myself down and wrote down what I observed until I got tired."

A fourth, in his frustrations, simply spent his time working through documented materials to find out what exactly was supposed to be happening.

The first two of these researchers had had experience and training in qualitative field research and so were not surprised that the "real behaviors" did not nicely fit the expectations of the research question. The last two had no such experience and found the unexpected field realities frustrating. One because he had been trained in a research tradition, that defines goals and then sets out to explain explicitly how they are attained, and the other because his background included no training or experience in research.

In the matter of training and background relevant to defining the research task and role, the nine DTA site researchers fall into four categories.

Group I consists of two researchers with academic backgrounds in anthropology and some ethnographic experience.

Group II comprises two researchers with training in educational administration and journalism respectively, both of which included exposure to research methods similar to ethnography. The first had used field research techniques in a graduate methodology course and the second had done extensive investigative reporting.

Group III is composed of two individuals schooled in psychology whose approach to research is radically different from that of ethnography. One, holding a behaviorist perspective, was loath to consider anything be,ond

observable behavior as data. The other had a similar aversion to inferring anything that could not be verbally confirmed through interview.

Group IV consists of two researchers without formal research training. Both had backgrounds in community social action, one held an MSW.

The first two researchers in the above examples were members of Group I, the third of Group III and the fourth, of the last. The backgrounds significant to these groupings, not only determined the researchers' approaches to perceived TAG shortcomings, they were crucial in defining the research task.

Two of the type I researchers spoke directly to this past project interviews:

> I had done ethnographic research before and knew that what I was going to do from the beginning. The process was intuitive because of my background.
>
> Because of my qualitative research experience I knew how to generate my own boundaries.

All three of these investigators took the anthropological stance that everything is data for something. The critical problem is deciding what that "something" should be and their background and experience provided the guidelines and confidence to make that decision.

The backgrounds of Group II researchers both of whom documented what they perceived as successful TAG strategies, provided equally ready models for engaging the task. However, the ensuing task definition was qualitatively different. It was less focused and they both collected large volumes of information which created problems of organization and writing. Both took detailed notes on every meeting they could observe and one

supplemented these with large numbers of taped and transcribed interviews. Neither considered the task, which they conceived more as reportage than analysis, to be outrageous.

The two researchers in the third group had unique and unusual experience. One, a doctoral candidate in experimental psychology, was hired to document a project which explicitly rejected the assumption of B.F. Skinner, the "reigning king of experimental psychology", and which was based on the humanistic premises of Carl Rogers. The other was a Ph.D in experimental psychology, investigated a TAG directed by a like minded clinical psychologist with whom he had worked.

Both defined their assigned tasks as ethnographic, and both explicitly expressed ignorance as to how to do it. Neither was able to rely upon their previous training or experience since both TAGs set affective, non-measureable (in experimental psychology terms) goals. One eloquently explained this dilemma:

> What it means for that program to have an effect was not spelled out by the folks... This is the essential difficulty of a behavorist working with a humanistically oriented group.

Neither researcher received much practical help from their supervisor. One was admonished to stay "naive" so as not to bias his conclusions. His reaction was, "if you are naive, everything's important!" The other simply found himself neglected because of operational problems in the project.

Both were gifted and facile writers. One produced a case study that was well-written, carefully organized and pleasing to no one. The second, did not put together a case study but wrote a number of interesting vignettes based on his observations in the target schools.

Neither of the researchers in the fourth group produced finished case studies. They had been hired with the understanding that they would be trained and carefully supported, in keeping with DTA's ideological commitment to provide "on the job training." Their failure to complete the case studies witnessed more an underestimation of the requirements of this kind of training than a flaw in conception.

Both had to virtually start from scratch. They confessed problems in understanding such basics as writing FDFs and the rationale for data coding. In each case, given problems with the ongoing support provided by the project, they had gained noticeable research skills. Pressures to finish the products did not allow adequate time for the process to continue.

When it came to adjusting the non-participant-observer role, factors additional to training and experience proved to be important. These included personal feelings toward participants in the target projects and how sympathetic researchers were to project goals. The following summary of researchers' own reflections illustrates this:

> One researcher, who was in sympathy with what was transpiring in the school, when asked took on the informal role of information processor when conflict slowed down proceedings. She professed care in not taking sides.
>
> Another, who maintained relatively formal relationships with site participants, knowing the content area and feeling positive about the TAG strategy, would frequently discuss mutual concerns on a theoretical level.
>
> A third, in sympathy with problems faced by certain parents with children in the school, was careful not to take a partisan stance during working hours, but felt free to serve as an advocate informally advising parents when she was not working.
>
> A fourth, who had strong feelings about effecting educational change and what was taking place at the school, scrupulously kept his feelings to himself until he had finished the research, at which time he had a long discussion with the principal during which he openly expressed his feelings.

Most of the others, in the interest of objectivity, deliberately maintained a relatively formal distant relationship with the site. None assumed an active role in either the TAG or a target school.

In summarizing this discussion, three facts stand out. First, despite the presence of a predefined research task and researcher role, these will be redefined in predictable directions reflecting the training and background of the individual researcher. Second, while the notion that contracted research can at the same time train researchers from scratch and produce quality results is conceptually feasible, it calls for important commitment of resources and the provision of adequate time for learning and then utilizing the new skills.

Finally, and most interesting, is the effect training and experience play in the quality of the experience for researchers. None of the four groups produced unequivocally better products than any other. Group I, II and IV researchers found the experience more personally rewarding than did those in Group III. For the first two it was simply a pleasant, relatively frustration-free job and for the inexperienced researcher it was a valuable learning experience. The third group, those with extensive training in a radically divergent research tradition, found the experience frustrating and little conducive to personal or professional growth.

2. Investment in the Research. A troublesome question faced by any contractor needing to hire field researchers is, "How, in the absence of the rewards offered in academia, to insure a personal investment and a working style compatible with the unusual demand of field researchers?" These demands are not congruent with the traditional ethos of day labor. Significant activities, for example, take place outside the normal nine to five

working day and doing really good research demands the kinds of personal investment that cannot be assured by any type of extrinsic supervision.

DTA was cognizant of this problem. It took extraordinary pains to employ site researchers who would take their tasks seriously. In doing this they down played the importance of experience in qualitative research as a consideration in hiring and emphasized potential for building rapport and knowledge of urban schools. An examination of the kinds of people hired suggests several possible motivations for extraordinary investment in the work.

Several were committed ideologues who, presumably by definition, are not simply motivated by wages. Others were admitted advocates of the TAG strategies they were documenting. Graduate students and new Ph.D's were put under contract with the tacit understanding that what they did would contribute to their academic or career goals.

The complexity of the project and the lack of research products that were clearly differentiated with respect to quality or usefulness, makes it impossible to definitively assess the effects of any of these potential motivational factors. Differences in the qualities of the research experience for the documentors does, however, shed light on some of the considerations important in making use of these potential resources.

The ideologues found the operational difficulties and the necessity to frequently compromise discouraging of their attempts to improve schools. Advocates discovered that the sites and TAGs were staffed by individuals of so many different persuasions that strong advocacy of one position brought them in conflict with another. At least two declared that they had "given up" and started to view their role as simply hired hands. Some graduate

students did find ways to use the DTA experience in furthering their goals by using DTA data for dissertations. How this affected the kinds of data they collected is impossible to discern given the flexibility of the DTA research design.

At least four of the researchers indicated that they felt their salaries were too low given their skills and the job they were expected to perform. One made this assertion by the way of explaining why he did not throw himself into his work, the other three in pointing out that they weren't simply working for wages. These feelings existed despite the fact that salaries were competitive with those paid new Ph.D's in academic settings.

Despite the disappointment at sacrifice of ideological purity, the discomfort at being caught in the snares of advocacy and the general sense of being underpaid, willingness to work was not an issue. There is only one demonstrable case (out of nine) where the field researcher consciously shirked his responsibilities. The problems came in assuring uniformly high quality products, in the collection of comparable data and in providing satisfying field experience for the researchers.

While these findings underscore the problem posed at the beginning of this section, they do warrant some tentative observations. DTA largely succeeded in securing the services of highly motivated, conscientious field staff through a combination of careful hiring and the provision of a flexible research design. This entailed some costs, however. The disillusionment, and consequent frustration suffered by several researchers being one, and disappointment with the useableness of the field data to the original goals of the project, being another.

Researcher-site relationships. Our examination of the process of building site relationships on the part of the researchers revealed no startling surprises. As is clear from past experience it is highly idiosyncratic. Several factors made it impossible in DTA for field researchers to make use of several resources traditionally found important in this process. They were not allowed to assume participant roles; for example, they couldn't capitalize on pre-existing personal friendships and they were under severe time constrains, they couldn't just hand around until people got used. Discovered they were, after all, rather harmless.

In light of these constraints, which will, in some form, attend any field research contract, the major factors making the DTA researcher-site relationships what they were fall into two categories. Researcher Characteristics Affecting Site Relationships. The most obvious set of character affecting site relationships are personal. In DTA care was taken to insure that the race and sex of the researcher would not be a barrier to establishing a fruitful relationship. In the final analysis neither seemed, except for initial contacts, to have been a very important factor. Other, non-ascribed statuses, carried more weight to the site people.

Among these, it turned out were professional characteristics. Site people were concerned that researchers be qualified to help them, in some cases have respected credentials, and not bring threatening or biased professional perspectives. In one site the training in Skinnerian behavioral psychology was welcomed as a potential aid. In another the researcher had trouble getting access to the inner working of the TAG because the local project leadership, who had lobbied for one of their in past consultants,

did not respect her credentials. In several cases the local people feared that the researcher's past experience and training had led them to a position of animosity to the project's strategy.

Perhaps the most surprising finding was the effects philisophical fit or misfit had on site relationships, reviewed in Chapter II. The problems erupting because of this were in part a function of the time and other constraints that did not permit the sites and the researchers to fully work them out. The surprise was that the problems did not involve the cognitive screens expected from ideological incompatibility, so much as cognitive dissonance the situations created in the researchers. Because of perceived pressures to come up with positive findings and the frustration of being impotent non-participants, several researchers found it difficult to comfortably document what they saw as weak or failing intervention strategies.

1. Strategic Stance. DTA, being as it was an admixture of action and traditional research, created unusual problems for researchers in defining the perspective they had to take toward the site. The original design called for an inside and an outside perspective with advisors from the sites providing the inside view and the researchers the outside. When the inside research panel idea was dropped, the role of insider devolved by default to the field researchers, even though they were precluded from assuming participant roles, and the research coordinator the outside.

According to the field researchers, this schizophrenic role was only one problem in establishing a strategic research stance. They had to decide how to divide their attention between the local project and at least one school site, which were frequently at odds with each other, and

they had to decide whether, and what kind of feedback to give the sites. A basic tenet of action research is that researchers give in return for what they get. Furthermore, most of the sites had requested feedback. However, throughout the project the relationships researchers enjoyed with the sites were so precarious or liminal and the information so piecemeal and tentative, that feedback entailed serious risks to the relationships even when mediated through research coordinators.

DTA researcher's responses to the problem of establishing a useful strategic stance toward the sites were highly varied. Some focused on school sites and some on the TAGs while others attempted to equally divide their time. Some carefully avoided any kind of feedback; others only provided it "after hours" or on a theoretical level. Some found it easy to get inside information and to take an insider perspective; others had to constantly fight to gain access to, what they saw as, vital information.

The lessons to be drawn from this review of researcher-site relationships in DTA are simple. Although action (or quasi-action) research may have potential advantages, functional equivalents to the boundaries and safeguards established in more conventional research cannot be neglected if cooption and personal frustration are to be avoided and a strategic research stance is to be maintained. In these processes, personal, professional and philosophical fit between the researcher and site are all important.

Research-contractor relationsship. Many of DTA's research related problems stemmed from the undefined and shifting quality of the relationships between it, as contractor, and the field researchers. Again, most of these have been treated already and simply remain to be highlighted.

A major issue was the ambiguity in defining role and task expectations for researchers. Parallel trends can be seen with respect to both roles and tasks. Originally researcher roles were seen as limited to information conduits. Later this was expanded to include important policy making and still later, when the expanded roles proved unwieldly and unworkable, they were de facto limited. The original task loads were modest. As the project developed they grew enormously until it became clear that none of them were being successfully accomplished. Toward the end of the program they were cut to essentially producing case studies.

A second issue was the unclear relationship model. Were researchers hiring research assistants or were they professionals contracted to do a largely self-defined project? If they were professionals, what model of professionalism is appropriate? Settling on a position has far reaching implications for tendering and receiving direction, devising appropriate models of supervision, assigning responsibility for field decisions and accepting responsibilities for results.

In DTA, the issue had two main facets. First, although it adopted a "professional" model this was not clearly defined sui generis nor did the project fit nicely into any established tradition holding a conventional definition. As a result, researchers were sometimes seen as university-type professionals and some times as public shcool teacher type professionals. Second, several of the hired researchers did not have the skills and experience expected in kinds of professionals they were treated as.

In Chapter II we have already reviewed the significance of these issues to the DTA experience. By the end of the project a majority of the field researchers had decided to adopt the posture that they were really employees and not truly professional researchers. For some this represented the attitude

with which they originally approached the task. For example,

> I was clearly a research assistant, a data collector. I didn't design the research. That's what makes a person a personal researcher.

For others this stance developed during their research to accomodate, what they saw as, unprofessional treatment by their supervisors or non-professional demands of the job. For all, the undefined relationship they enjoyed with the project as a whole, was a matter of continuous concern.

From the standpoint of the contractor the issues of researcher role and status, took a curious twist. It had to make the practical decision as to whether to view researchers more as part of a team, with the responsibilities and rewards for their work to be shared or as semi-autonomous investigators. The project expressed ambivalence in this regard throughout. Gradually, however, as the organization evolved (ironically, from a task to a team orientation) each documentation site came to be treated more and more as a semi-autonomous unit with developments, and even personnel changes, being made in one having little effect on the project as a whole. Concommitant with this development researchers evidenced a growing suspicion that they were going to be judged by their case studies alone and directed their energies accordingly.

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APPENDICES

APPENDIX A: JOB DESCRIPTION FOR SITE RESEARCHER

JOB DESCRIPTION FOR SITE RESEARCHER

I. Qualifications

A. Skills and experience with qualitative and quantitative social science research.
B. Experience in urban schools and familiarty with the project area.
C. Sensitivity to issues of rapport building.
D. Graduate training in education, anthropology, sociology, or psychology, or equivalent experience.
E. Willingness to stay in the job at least until December 1977. CNS/CEPM will give preference to candidates who wish to move into other phases of the D&A project after December 1977. The D&A project ends in December 1979.

II. The selection process for the Site Researcher will be the following:

A. Candidates may be nominated by both CNS/CEPM and the local project.
B. CNS/CEPM will conduct initial screening of the candidates. The local project if interested, will be involved in further screening of the candidates.
C. CNS/CEPM will select the Site Researcher from the pool of nominees.

III. Responsibilities

A. The Site Researcher will be the chief collector of information for CNS/CEPM and liaison between the Local Project and CNS/CEPM.
B. The Site Researcher will neither be a detached outsider nor an active participant in the Local Project.
C. The Site Researcher will develop a sufficient understanding and sympathy with the Local Project to protect it against insufficiently informed conclusions from data in the offices of CNS/CEPM and at the same time maintain a high respect for objective data that will enable conclusions to rest upon demonstrable facts and not merely sympathy.
D. The Site Researcher will develop his role so he is not perceived as an advocate of any constituency within the Local Project.

IV. Evaluation of performance

A. The Site Researcher will be evaluated initially after a three-month period and thereafter at least annually.
B. Evaluation will be carried out by CNS/CEPM with input from the Local Project.
C. The following evaluation criteria will be used:

1. Continued acceptance by the varied constituencies of the Local Project as judged by members of the Local Project.
2. Provides orderly, precise, and interpretable data to CNS/CEPM.
3. Maintains communication with local people that elicts questions and issues that are relevant to their perceptions of what is important.
4. Maintains communication with the Research Coordinator that can counter theoretical notions with facts from the site.

5. Abides by the agreements which have been negotiated by the Local Project and CNS/CEPM.
6. Manages financial responsibilities and other administrative details adequately.
7. Carries out effectively the other responsibilities listed under III.

APPENDIX B: WHAT'S COMING UP FOR SITE RESEARCHERS/ GENERAL ISSUES

MEMORANDUM

TO: All Site Researchers

FROM: Steve, Research Coordinators

DATE: March 16, 1976

RE: What's coming up for Site Researchers/General Issues

In Eugene, Site Researchers were promised a list of events and expectations. We've tried to put one together that will give you some idea of what's coming up. You should note several things about this list:

1. It's tentative. You probably realize now that DTA is a developmental project, so it's often impossible to specify exactly what's going to happen. The list should give you a fairly good idea. We cannot guarantee, however, that this is all there will be or that it will take this form exactly.

2. It's especially hard to specify expectations far in the future. For example, we are not sure at all what Task 3 needs will be beyond January, 1976, nor what will be the relation of Task 3 work to cross-site analysis. Similarly, the period beyond June, 1976 is necessarily vague at this time. Planning for January - June will probably begin in November and Site Researchers will be involved.

We also wanted to comment on some other unfinished issues:

1. Jim Taylor has already contacted you about business cards. These should be coming soon.

2. Terry has contacted you about criteria of evaluation for Site Researchers. A final version will be coming.

3. NIE is getting us the general memos on refunding they sent out to sites. We will be sending these to you.

4. We are working on guidelines for the old inside research panel money (now research support money).

5. New York Site Researchers are working on a memo to formalize Site Researcher communication arrangements and Site Researcher participation in planning.

6. You should have received draft of policy guidelines. Please send reactions back as quickly as possible.

7. All Site Researchers should be thinking about potential site advisors.

8. If not already done, Site Researchers should be thinking about distribution of effort issues--if there are more than two schools, how will you choose which ones to focus on?

9. The computer system is getting in final form. We will be sending you a revised codebook soon.

10. If you have not already done it, please send Steve your reflections about the Eugene Site Researcher Workshop.

11. We hope to write some answers to Site Researcher questions on method that came up at Eugene. They were not answered by Wolcott.

12. There was some discussion about books to be sent to Site Researchers. One was Wolcott's special issue of Human Organization on ethnography of schools. We're trying to get a copy in order to review it to see if it's relevant. Also we'll be developing a book policy.

13. Personal statement: All Site Researchers were requested to write a statement describing their personal theories about education and organizational change.

14. School is about to start. Our best wishes are with you.

APPENDIX C: LIST OF EXPECTATIONS

LIST OF EXPECTATIONS

Weekly	Send two copies field data forms to Research Coordinators. Discuss field data forms and other research and site issues with Research Coordinator.
Twice a Month	Send in time sheets to Ada Skyles/CNS.
Every Two Weeks (Maximum)	Talk to Task 3 link person to find out what Task 3 is doing, to suggest issues and to respond to questions (with prior notice). For details about all Task 3 activities, see special August, 1975 memo from Steve about linkage between Task 1 and 3.
Monthly	Talk to NIE project officer. After refunding activities are over, the conversations may not be as frequent.
Week of September 8	Respond via telephone to priority Task 3 questions needed for entry (See second memo from Roy Poole). Presumably Site Researchers will have already gathered much of this information during the summer emphasis on history.
End of September	Analytic paper due on the history of the site. Detailed guidelines are soon forthcoming. Approximately every two months Site Researchers will be expected to write short analytic papers on topics agreed upon by Site Researchers and Research Coordinators. These papers will serve many purposes: 1. They will systematically build a basis for the case studies so that there isn't an impossible task at the end. 2. They will insure some commonalities of subject across case studies. 3. They will provide a vehicle for Site Researchers to enter their analysis into the system in a way not allowed by field data forms.

4. They will provide a way for Site Researchers and Research Coordinators to share data and research ideas.

October	Site Researcher position paper due describing background of Site Research and attitudes toward educational change in general and the strategy of the site in specific.
	Visit by Research Coordinator. (feedback)
Sometime October-December	Visit by Task 3 Link Person (date will be negotiated).
End November	Analytical paper (topic to be discussed).
December-January, 1976	Cross-site visits: each Site Researcher will visit another site and be visited by another Site Researcher. Research Coordinators will also visit one site for which they don't have primary responsibility.
End January	Analytic paper: probable topic will be cross-site focus suggestions in preparation for cross-site conference.
Middle February	Cross-Site Conference.
March	Visit by Research Coordinator. (feedback)
End April	Analytic paper: topic to be determined.
May-June	Visit by Research Coordinator.
End June	Analytic paper: topic to be determined.
Sometime January-June	Visits by Task 3 people (?)
September, 1976	Rough draft of case study due.
September, 1977	Final draft of case study.

January-June, 1977	Possible visit by DTA principal investigator. Possible visit by Jim Taylor for intensive media work (this would not happen at all sites). Possible special trouble shooting visits by Research Coordinator.